THURSDAY, MARCH 31, 1881

MIND IN ANIMALS

Mind in Animals. By Prof. Ludwig Büchner, author of "Force and Matter," &c. Translated, by the author's permission, from the German of the Third Revised Edition, by Annie Besant. (London: Free-thought Publishing Company, 1880.)

THE translation into English of Büchner's work on "Mind in Animals" (which was originally published in 1876) cannot but be welcome to persons of all shades of opinion, however greatly opinions may differ as to the auspices of the Company which has undertaken and published the translation. The Company, among other things, undertakes the translation of works from foreign languages in the form of a series entitled "The International Library of Science and Free-thought," and of this series Büchner's work on "Mind in Animals" constitutes the first member. It is to be hoped that the subsequent efforts of the Company in this direction will prove as useful and beneficial as the one we are about to consider. The translation has here, on the whole, been well done, although occasionally we meet with an awkwardness of construction which a little more care in re-casting the sentences might easily have obviated. The work itself is without question a highly valuable compilation of facts relating to Comparative Psychology, and therefore its translation into English supplies a fitting occasion for our endeavouring to recommend it to the notice of English readers.

Although the work is called "Mind in Animals," and fills between three and four hundred closely-printed pages, it only deals with the psychology of articulata, and even of this comparatively limited group it treats only of four orders, viz. the *Hymenoptera*, *Orthoptera*, *Arachnida*, and *Coleoptera*. No one, however, can read the work and feel that this limitation of its subject-matter is a defect, although in view of it the title might perhaps have been appropriately changed to "Mind in Insects."[1] As he says in his preface, "the author has not thought it necessary to widen the circle of his observations over the whole of the comparatively narrow and yet infinitely wide and rich sphere of intelligent insect life; he considers it better . . . to treat a single species thoroughly, rather than many species cursorily and superficially," &c. Such being the author's aim, he appears to have read most of the existing literature upon the subject that is worth reading, and then made a compilation, tolerably well sifted, of all the more important facts. These he has presented in a form at once highly entertaining to a general reader of the lowest intelligence, and most useful alike to the working naturalist and the evolutionary psychologist. The labour represented by the result is very considerable, and Büchner deserves all thanks and praise, both from the scientific and non-scientific public, for the patient industry with which, like the ant or the bee that he is so fond of upholding as a model, he has collected and arranged his materials.

More than three-fourths of the book is devoted to ants and bees, and this portion constitutes a compendium of facts regarding the psychology of these interesting animals which we do not hesitate to regard as the most instructive that has hitherto been made. There are however no original observations in the book—or rather no original observations recorded as made by Büchner himself, for there are several highly interesting observations recorded as made by friends and correspondents. Some of the more important of these we may here present.

Herr Lehr, a "bee-keeping friend of the author," noticed that when his bees were attacked by dysentery, and "no longer able to retain their excrements, one hive suffered less than the others." Investigation showed that the bees of this hive had made a drain from the upper part of the hive, "where they were accustomed to sit together during the winter," to the exterior.

The same (?) friend observed that when the wind blew down one of his hives and he replaced it, a few days later "the bees had left their old home in the lurch, and tried to enter other hives, clearly because they could no longer trust the weather, and feared that the terrible accident might again befall them."

Another friend, Herr Schlüter, saw a hornet catch a cicada, sting it, and try to fly off with the bulky prey. The hornet's strength not being sufficient to enable it to fly with the cicada from the ground, it dragged its burden up the stump of a mulberry-tree that stood close by. Arrived at the top of the stump—twelve feet from the ground—"it rested for a moment, grasped its victim firmly, and flew off with it to the prairies. That which it was unable to raise off the ground it could now carry easily once high in the air."

Again, Herr Merkel communicates the following. He saw a little grey wasp dragging a long caterpillar to its hole. Arrived there, it put one end of the caterpillar over the hole, "and went to the other end and lifted it up so high that the caterpillar fell in. But a piece of it stuck out"; so the wasp pulled it out, and, laying it down near the hole, "went in again and brought out several little stones of the size of small peas. It then again let the caterpillar fall into the hole in the way described." This time it was quite absorbed by the hole, and was buried by the wasp.

More interesting is an observation due to Herr Nottebohm, Inspector of Buildings at Karlsruhe, who carefully syringed off all the aphides from a weeping-ash in March, to the great benefit of the tree. But in June he was astonished to see multitudes of ants running up and down the trunk of the tree, busied in carrying up aphides all over the tree in order to re-stock it, and "after some weeks the evil was as great as ever."

Again Herr Theuerkauf showed Büchner a maple tree, round which he had "smeared about a foot-width of the ground with tar," in order to check the mischief caused by ants cultivating aphides. But the ants on the tre turned back on finding the tar, and "carried down aphides, which they stuck down on the tar one after another, until they had made a bridge over which they could cross the tar-ring without danger."

Highly interesting also is an observation communicated to the author by Herr Kreplin concerning the Ecitons crossing streams, which is even more wonderful than anything that has been related of these wonderful insects by Bates or Belt. "If no natural bridge is available for the passage, they travel along the bank of the river until they arrive at a flat sandy shore. Each ant now seizes a bit of

[1] The Arachnida are called by Büchner insects, in accordance with popular usage.

dry wood, pulls it into the water, and mounts thereon. The hinder rows push the front ones even further out, holding on to the wood with their feet and to their comrades with their jaws. In a short time the water is covered with ants, and when the raft has grown too large to be held together by the small creatures' strength, a part breaks off and begins its journey across, while the ants left on the bank busily pull their bits of wood into the water and work at enlarging the ferry-boat until it again breaks. This is repeated as long as an ant remains on the shore."

Similarly, Dr. Ellendorf informs the author that he has witnessed ants using a straw for a bridge across a saucer of water which he had placed as a barrier between the ants and his provisions. He then pushed the straw about an inch from one of its two landing-places. After much confusion and crossing of atennæ, the ants "soon found out where the fault lay, and with united forces they quickly pulled and pushed the straw until it again came into contact with the wood, when the communication was again restored."

The same observer communicates another very interesting observation on the leaf-cutting ants. He interrupted a marching column by placing a withered branch across their road. The loads were laid aside by more than a foot's length of the column, and the ants began on both sides of the branch to tunnel beneath it, and when the tunnel was completed "each ant took up its burden again, and the march was resumed in the most perfect order."

These being the most important additions which Prof. Büchner's work has made to our previous knowledge of insect psychology, we shall now proceed to make a few criticisms upon the work as a whole. In the first place, the author is not quite free from the failing common to less critical writers on animal intelligence, of admitting dubious cases without sufficient reserve. Thus, for instance, on no better authority than Plutarch, he gives (p. 57) a case "related by a certain Cleanthes," of ants going from one ant-heap to the entrance of another, carrying a dead ant. Other ants came out of the visited heap, consulted with the bearers of the body, went back again and brought a worm "out of the depths of the nest, which was evidently intended to serve as a ransom for the dead body. Then the ants which had brought the corpse left it lying there, and carried away the worm instead." He then adds, "However incredible this may sound, it is beyond doubt that ants and bees have been seen carrying away and even burying their dead, and of this further details will be given later." As the fact of "burying" is highly dubious, we looked forward from this statement to afterwards meeting with some new evidence upon the subject; but in the case of ants only found the unsupported assertion of Dupont, followed by a confusion of the well-ascertained fact that ants carry their dead away from their nests, with the inference that they bury them (p. 167), while in the case of bees we only met with (p. 249) a very flimsy anecdote, which we had previously read in Watson's "Reasoning Powers of Animals," quoted from the *Glasgow Herald* on the authority of an anonymous correspondent; it presents a pathetic account of two bees flying out of a hive "carrying between them the corpse of a dead comrade," till, after searching for a suitable hole, they "carefully pushed in the dead body, head foremost, and finally placed above it two small stones. They then watched for about a minute before they flew away"—no doubt, of course, performing some appropriate funeral service. And this is the evidence on which the earlier statement rests, "*it is beyond doubt* that ants and bees have been seen . . . burying their dead"! Such cases of careless judgment, however, in admitting alleged facts on wholly inadequate evidence, are fortunately in this work exceptional.

Another point on which criticism has to be offered is the frequent failure of references. Important facts are constantly stated without any information being supplied as to the authority on which the statements rest. Again, even when the authority is stated, after the first time of quoting the reference is always to *loc. cit.*, so that unless the name of the work is carried in the reader's memory, he has to hunt back through an indefinite number of pages of letterpress till he finds it.

Another feature of the work which must be considered a blemish upon it as a work of science, is a perpetual breaking out of allusions to matters religious and political. The strong bias which the author displays in these digressions, apart from being singularly out of place in a treatise which aims at scientific method, constantly leads him into obvious fallacies. For instance, when speaking of ants, he asks, "Why should we take it for granted that in a perfectly free community men would only work if compelled, when these animals give proof that such a free commonwealth is very possible, and is compatible with the voluntary work of all?" Certainly any one who is disposed to take such a supposition for granted, would scarcely be convinced by such a false analogy as that between an ant and a man; and he might very easily show up the nonsense by replying, "Why should we take it for granted that men in a perfectly free community *would* work without compulsion, when the grasshoppers give proof that such a free commonwealth is very possible, and is compatible with no work at all?" Such is the logic of many of these passages, and we do not think that in others of the same kind the sentiments are much more fortunate. It is, for instance, to be doubted whether the following picture of "the widest Socialism and Communism" as revealed in bees, and held out as an example for humanity to imitate, will prove as attractive to the eyes of all his readers as it evidently appears to the eyes of the writer. "They have no private property, no family, no private dwelling, but hang in thick clumps within the common-room in the narrow space between the combs, taking turns for brief nightly repose" (p. 266). On all such matters opinions may legitimately vary; but allusions to them are, as we have said, out of place in a treatise on Comparative Psychology.

Coming next to criticisms of a more purely scientific character, we have first to notice a meagreness with which the whole subject of instinct is treated. In his anxiety to combat the supernaturalists, Büchner errs on the side of too closely assimilating the psychological faculties of insects with those of men. That is to say, he endeavours to explain most, if not all, instinctive action as being one with "reason" and "reflection." But it is an enormous and damaging mistake in the cause of evolution to disparage the distinction which unquestionably exists between mind in animals and mind in man. The function of an historical psychologist is to explain

the origin of instincts and the development of rational thought—not to slur the two together as presenting but little difference to be explained. Yet in two chapters devoted to instincts we have in this treatise scarcely a word to explain their probable mode of origin, and nothing to show how they may be supposed to have developed into reason. This "inverted anthropomorphism" constantly leads the author into statements which are little less than absurd—as, for instance, when speaking of the wedding-flights of bees he observes that their leaving the hive to copulate in the air "seems as though a feeling of modesty prevented the queen from performing this act before the eyes of the crowd."

Again, even in the few places where he does touch upon the origin of instincts, his treatment of the subject is most unsatisfactory. Taking, for example, his remarks on the difficulty presented by the case of neuter insects being derived from parents which display totally different instincts from their progeny, he adopts the view that fertile females were originally workers, lost their working instincts by degrees, but now leave them as perpetual legacies to their barren offspring. Now, although this view may be taken as a mitigation of the difficulty, it certainly cannot be taken as a full "answer" to it. Büchner very lightly passes round a mountain of trouble where he says, "that this opinion, if correct, would also apply to the other social insects, and especially to ants, scarcely requires special argument." This is a most astonishingly complacent way of eluding what Darwin calls "the climax of difficulty" which is presented by several castes of workers having instincts differing, not only from their fertile parents, but from one another. The truth is that the theory advanced by Büchner is alone clearly inadequate to meet the facts; and he does not appear even to have read, or else to have entirely forgotten, the gem of condensed and candid reasoning upon this subject by which the beautiful theory concerning it is rendered in the "Origin of Species."

Lastly, even as to matters of fact there are some criticisms to be made. A serious sin of omission is to be complained of in the description of the habits of the leaf-cutting ants, in that no allusion is made to the theory of Bates—which having been since supported both by Belt and Müller, deserves to be regarded as highly probable, if not virtually established—concerning the object with which the leaves are cut and garnered, namely, to grow fungi upon. Again, in dealing with the so-called agricultural ant the author is, we think, somewhat too definite in his statements as to these insects planting seed. So far the remarkable story on this head rests on the unsupported authority of Dr. Lincecum (not Linecum, as repeatedly misprinted), and although it may prove true, ought not, until amply corroborated, to be thus unreservedly accepted.

Other criticisms of the same kind might be passed, and we cannot help feeling it would have been well to have added a short chapter to the translation bringing the literature of the subject up to date, and likewise an index; but enough has been said to signify our general estimate of the work. In all matters of fact it is, as a rule, most accurate and comprehensive. In its philosophy it is not strong. But as a whole it is a decidedly valuable addition to the literature of Comparative Psychology.

GEORGE J. ROMANES

AMERICAN INDIAN LANGUAGES

Introduction to the Study of Indian Languages. By J. W. Powell. Second Edition. (Washington: Government Printing Office, 1880.)

THIS is one of the most useful of the many useful works issued under Mr. Powell's able management by the ethnological bureau of the Smithsonian Institution. It was originally published in 1877, and it is satisfactory to find that another edition has so soon been called for. At the same time one cannot but regret that this opportunity was not taken to somewhat modify the title, which, as it stands, is apt to deceive the unwary. The book is in no sense an abstract treatise on the nature, structure, or classification of the American languages, either regarded independently or in relation to other forms of speech. It has nothing to do with the philosophy, or even with the grammar of these idioms taken collectively or individually. Its object, if less ambitious, is perhaps far more useful in the present state of these studies. American philologists have confessedly shown a disposition to dogmatise on the morphology of the native idioms, and have indulged in some very wild speculation on utterly insufficient data regarding their origin, development, and affinities. The old school of etymologists, who held that Eliot's Massachusetts Bible was merely a thinly disguised form of Welsh, that Delaware and Lapp were first cousins, and that Basque sailors stranded on the Brazilian seaboard could hold converse with the Tupinambas and other Guarani peoples of that region, has had its day. But it has been succeeded by another, which, if slightly more cautious, is scarcely less extravagant, and which, notwithstanding the warning voice of science, still flourishes in both hemispheres. It will suffice here to refer to the astonishing theories seriously advocated by the late Abbé Brasseur de Bourbourg on the relations of the Maya-Quiché and Aryan families, by the Abbé Petitot on the Athabascan and Chinese, and quite recently by Mr. John Campbell of Montreal on "The Hittites in America." "The Aleutans and Barabra," writes the last-mentioned authority, "agree in being worshippers of the sun like other Hittites, in the manufacture of red waterproof leather, and in their manner of adorning the head. . . . Physical ethnology would never have dreamt of uniting white Basques and Circassians, black Nubians, yellow Japanese, and red American Indians; but philology, which knows no colour but that of words and constructions, makes them one. It may be that in the Barabra we shall yet find the purest surviving form of the ancient Hittite language. Some of its numerals help to connect those of the Peruvian dialects with other Hittite forms." One thing more surprising perhaps than such insanities is their appearance in the pages of a professedly scientific journal (*The Canadian Naturalist and Quarterly Journal of Science* for August, 1880, p. 359).

A wholesome check to writers inclined to indulge in tendencies of this sort is afforded by the modest and unpretending character of the work under consideration. It is put forward simply as a guide and aid to students in the collection of linguistic materials in a very wide field, where the labourers are still too few for the urgent and extensive character of the work to be performed. It thus brings us back to the domain of hard facts, wisely

reserving all speculation for a time when these facts will have been accumulated in sufficient number to afford a sound basis for more general inductions. "The book is a body of directions for collectors" (Preface vi.). It is divided into three chapters, one "On the Alphabet," another containing "Hints and Explanations," and a third supplying a large number of forms or "Schedules" to be filled up by the collector. The chapter on the Alphabet aims at establishing some uniform system of spelling for all the native tongues, and puts forth a comprehensive scheme embodying many useful suggestions well deserving the attention of our "spelling reformers." These are summed up in a few fundamental rules, the chief of which are the exclusion of all characters and diacritical marks except those found in ordinary English printing offices, and the restriction of each sign to a single sound. The difficulty of adapting the Roman system to the Indian tongues will be understood when it is stated that "there are probably sounds in each which do not appear in the English or any other civilised tongue; and perhaps sounds in each which do not appear in any of the others, and further, that there are perhaps sounds in each of such a character, or made with so much uncertainty, that the ear is unable to clearly determine what these sounds are, even after many years of effort" (p. 2). Nevertheless the difficulty is manfully faced and largely overcome by the scheme here adopted, which is founded on one originally proposed by Prof. J. D. Whitney, and which is consequently at once scholarly, simple, and comprehensive. A few improvements might here and there be suggested, but on the whole there is little to complain of, except perhaps the use of the circumflex (^), to mark both a long *a* sound, as in *all*, and a short *u* sound as in *but*. Some confusion is caused by an awkward misprint at p. 5, where this *â* appears instead of the German *ü*. It might also perhaps be better to indicate excessive vowel length by doubling the vowel as in Dutch, than by the clumsy addition of the sign +. Thus *maan* rather than *ma + n*.

Chapter II. contains a number of well-digested and tersely-expressed remarks on dress, ornaments, dwellings, implements, food, colours, plants, animals, medicine, social organisation, kinship, government, and many other topics, which at first sight seem to have little connection with the subject of American philology. But the author has wisely endeavoured thus "to connect the study of language with the study of other branches of anthropology; for a language is best understood when the habits, customs, institutions, philosophy—the subject-matter of thought embodied in a language are best known. The student of language should be a student of the people who speak the language; and to this end the book has been prepared, with many hints and suggestions relating to other branches of anthropology" (Preface vi.). But besides these matters the chapter contains what will be welcomed as a boon by all linguists, a reprint of J. H. Trumbull's masterly paper "On the Best Method of Studying the North American Languages," originally published in the *Transactions* of the American Philological Association, 1869-70, but strangely neglected by many subsequent writers on the subject. No other treatise perhaps of equal length contains so clear and philosophic an account of the peculiar genius and morphology of these polysynthetic tongues. A great deal of space is devoted to the question of kinship, the true basis of Indian tribal society, and this intricate subject is fully illustrated by a series of four "kinship charts" or genealogical diagrams, which the original investigator will find of the greatest service in collecting and arranging his materials. The general student will also find them extremely useful in comparing the American systems of family relationship with those prevalent especially amongst the Dravidians of the Deccan and the Australian aborigines. Too much importance has perhaps been attached to resemblances of this sort in tracing racial affinities; but their significance in the history of the evolution of human culture is undeniable. Connubial society develops into kinship society, or the clan, in which all the members are blood relations, whence the tribe and nation. It is remarkable that the connubial, or lowest form, still so prevalent in many parts of the eastern hemisphere, seems to have long disappeared, at least from the northern half of the New World, although some of its customs, especially those associated with kinship, still survive in the more advanced tribal state. This explains the barbaric wealth of family nomenclature with which the Indian languages are still encumbered. In the printed forms, or schedules, of which Chapter III. exclusively consists, the terms of relationship occupy about forty pages, and include hundreds of complicate affinities such as, "my father's elder brother's daughter's daughter's daughter's daughter," "my father's mother's brother's son's son's son's son," "my mother's father's brother's son's daughter's daughter's daughter," "my mother's mother's sister's daughter's son's daughter's daughter," "my mother's elder sister's daughter's daughter's daughter's husband." For these, and even more intricate degrees of parentage, many native tongues supply equivalents, which the collectors are accordingly required to discover and insert in the blank columns prepared for the purpose in the schedules. The arrangement of the other matter contained in these schedules seems to be somewhat needlessly involved. At least the advantages are scarcely so obvious as the inconvenience of breaking up the strictly lexical part into upwards of twenty sub-headings, instead of lumping the whole in one general vocabulary arranged alphabetically. Experience has abundantly shown how troublesome is the use of such minutely-classified lists of words even for the compiler. This remark does not of course apply to the lists of sentences (Schedules 26-9), which appear to have been carefully prepared, and are well calculated to bring out the structure and varied grammatical forms of the Indian languages.

A. H. KEANE

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Hot Ice

THE letter of Mr. Perry (NATURE, vol. xxiii. p. 288) in answer to mine on the subject of Dr. Carnelley's experiment (p. 264) has remained a long time unanswered, partly because I

was led by the letter to suppose that Prof. Ayrton himself might have something further to say regarding his views as soon as he returned to England, but mainly because I did not see any point in it specially requiring an immediate reply. I find however that a considerable amount of cautious scepticism and suspense of judgment still prevail on the subject—a scepticism which Prof. Herschel's enthusiastic letter of a month ago (p. 383) has not gone far to remove, because, though there can be no doubt of his confirmation of the fact that ice in a hot vacuum is *infusible* and disappears slowly, there is nothing in his letter confirming the hypothesis that it is *hot*, which is the only point under discussion.

Now for my own part I fully and unreservedly accept this as a fact, not only on account of Dr. Carnelley's experimental evidence, but also because I imagine myself to perceive exactly why it occurs, and indeed that it might conceivably have been conjectured as probable beforehand.

My present communication therefore is merely to remove as far as possible any sense of mystification which Prof. Perry's letter may have tended to produce, and to indicate the ground of his error.

Professors Ayrton and Perry, with their stiff paper models, start, if I am not mistaken, on the assumption that the ordinary equations deduced from the two laws of thermodynamics will apply to the case: and this is exactly how I started myself. I considered that it was necessary to investigate the behaviour of a substance whose properties were defined, not by two independent variables, as is usual, but by three; the pressure, quantity of solid, and temperature, being all three arbitrary and independent of each other in the Carnelley experiment; and I extended Clausius's general equations to suit this case. But it was very soon evident that they did not apply at all, and for this reason, that the second law is only true for processes that are *reversible*, and the sublimation of hot ice is essentially an *irreversible* process. This is indeed the whole gist of the matter, and it is entirely due to this that the ice gets hot. Ordinary evaporation of a liquid below its boiling-point against a pressure less than its "vapour-tension" is an irreversible process, and accordingly the temperature is perfectly indefinite, and depends on the rate of supply of heat and on the rate of evaporation. So also with ice above the boiling-point, that is, ice subliming under a less pressure than the vapour-tension; its temperature depends simply on the rate of supply of heat and on the rate of evaporation. So far everything is perfectly simple and absolutely certain.

The only possible question that can arise is whether internal disintegration of the solid will not set in and prevent its rising above the boiling-point: whether in fact a solid cannot boil as a liquid does. I have given reasons for believing that in a solid formed *in vacuo*, or without air-bubbles, and constantly rising in temperature, this will not occur; and I deny that under these circumstances it is in a particularly unstable condition analogous to that of superheated water on the point of "boiling by bumping."

This however I fully admit is a point distinctly open to discussion, and I imagine that without an experiment one could not feel at all certain about it. But personally I feel that the evidence already given us by Dr. Carnelley, together with the theoretical probability indicated in my former letter (p. 264), is sufficient and conclusive.

It was no doubt somewhat staggering to learn (NATURE, vol. xxiii. p. 341) that Prof. McLeod, with his well-known experimental skill, should have hitherto failed to repeat the experiment, or to get the ice at all above zero;[1] but I take this as an instructive example of those rare cases where refined experimental appliances are obstructions rather than aids, for I believe the failure to be simply due to the fact that Prof. McLeod's vacuum was far too perfect, and the evaporation therefore so rapid that the ice did not have a fair chance of showing its willingness to rise in temperature; it could not in fact get even as high as 0° C. But if Prof. McLeod will discreetly spoil his vacuum until the pressure is only just below the vapour-tension corresponding to the temperature shown by his thermometer, I have no doubt that he will see the ice rise to any temperature he likes, and he will find that when it is crossing zero it will be utterly regardless of the fact.

The same kind of statement applies to solid carbonic acid, on which I have made a few experiments with a view to raising its temperature. I squeezed it into the ice form in a hydraulic press (to diminish the evaporating surface), put a thermometer in it, and held it over a fire. The evaporation is so excessively rapid, however, that it remains apparently just as cold as before.

I have not time to follow it up just now, but the obvious thing is to put it under pressure, so as to diminish the rate of evaporation, and then heat it. Prof. McLeod informs me that the boiling-point of CO_2 continues below its melting-point (which is given by Frankland as −57° C.), until the pressure is four atmospheres; so that anything just under four atmospheres may be applied to this substance with impunity, and it will then be exactly in the most favourable condition for the Carnelley experiment; and I have not the slightest doubt that it can then be warmed, and if at the same time the pressure be judiciously and gradually increased, that it can be made as warm as one pleases until it has all disappeared.

Experiment with substances other than water however are likely to be more difficult, simply because few substances have such a large latent heat both in the liquid and gaseous condition, and therefore few substances will be anything like so permanent and outlive the evaporation so long. OLIVER J. LODGE

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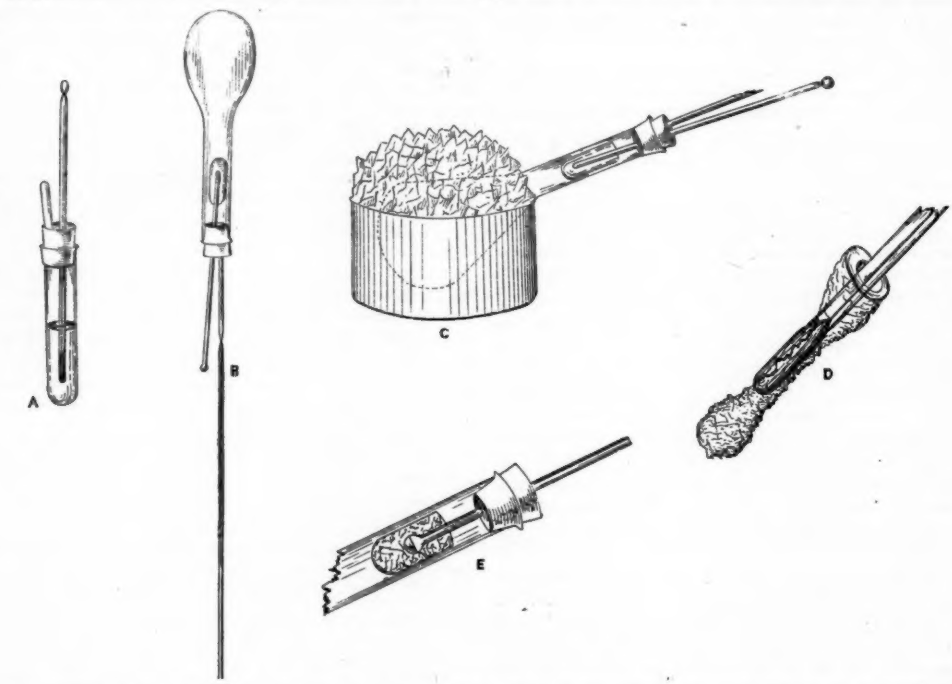
[1] Since this was in type I have received, by the kindness of M. Boutlerow, a copy of a paper read by him before the St. Petersburg Academy of Sciences, in which he summarises the views which have appeared on the subject, relates his failure to repeat the experiment, and confesses himself a sceptic. It would not be doing justice to M. Boutlerow's carefully-wrought memoir to discuss it in a foot-note, but it is my impression that his failure is due to the same cause as that which I have ventured to suggest above as accounting for Prof. McLeod's, viz. too perfect apparatus and too great experimental skill.

THE announcement made some time since by Dr. Carnelley that ice *in vacuo* could be raised to a temperature far above its ordinary melting-point, seemed so thoroughly in opposition to the experience derived from the great work of Regnault on the tensions of vapours; and as it called for a complete change of ideas in a field in which I am much interested, and as Dr. Carnelley asked others to repeat his experiments, I was induced to examine for myself the experiments on which so curious a statement was founded.

I used two different methods: the Torricellian vacuum and the Sprengel vacuum. As the experiment, as conducted by the Torricellian method, can easily be repeated by any one, and is much simpler in form than Dr. Carnelley's, I shall detail it. In the first place I wished to obtain a clear continuous piece of ice round the thermometer, as Dr. Carnelley's method gave flaky ice, which I found might lead to errors, owing to its discontinuity leaving the thermometer bare in parts. To obtain clear ice the following method was used:—Some distilled water was boiled in a test-tube A fitted with a two-holed stopper, with a thermometer through one hole dipping into the water; when all the air was expelled, a glass plug was pressed into the other hole against the issuing steam, and the whole allowed to cool, and then frozen in a freezing-mixture. A long-necked "German Florence flask" was then rinsed with distilled water and filled with mercury, and also placed in a freezing-mixture. The tube A was then gently warmed with the hand, and the plug of ice adhering to the thermometer withdrawn. The glass plug in the second hole in the stopper was then replaced by a marine barometer-tube of about forty inches in length, having been drawn out about four inches from the top to facilitate sealing. The plug of ice round the thermometer was then inserted into the neck of the flask full of mercury, and the stopper pressed home. This caused the mercury to rise in the barometer-tube, and the whole was then inverted as at B; and when the mercury had all run out, the fall tube was melted through at the constriction B, leaving a Torricellian vacuum above. The flask was now laid on its side in a freezing-mixture and well covered over with ice and salt as at C. After a few minutes, to allow the receiver to cool, heat was applied to the neck of the flask with a Bunsen lamp, and even with a blowpipe, till the glass softened, but the temperature of the thermometer did not rise until some part of it became denuded of ice, or until air had been admitted. The experiment was repeated again and again, but in no case while the vacuum was intact could the temperature of the ice be raised materially above that of the receiver. If the temperature of the receiver was −12°, then the ice was a little over −12°, say about −11°, but never more than two degrees above the receiver, although the glass almost in contact with the ice was at its softening point. This is exactly what we would expect from Regnault's experiments; the temperature of the receiver determines the vapour-tension, and therefore the "boiling point" of the ice. The ice was certainly never hot, and was not even

infusible, because when pressed against the hot glass it at once splashed out, freezing again in long thin flakes when it obtained free space for evaporation. All the heat passing to the ice is used up in volatilising it, and increase of the source of heat merely increases the rate of evaporation, as in the case of water boiling under atmospheric or other constant pressure; provided the condenser be efficient. These experiments were repeated with different thermometers and thicknesses of ice, varying from $\frac{1}{4}$ inch to the thinnest film, $\frac{1}{50}$ inch, or thereby, and the temperature of the ice was always dependent upon the temperature of the receiver (when vacuous) and quite independent of the temperature surrounding it; the latter merely determining the rate of evaporation. Whenever a hole appeared in the ice covering the thermometer the latter rose, and if close to the hot glass rose rapidly. When the ice wore away, as shown at D, the temperature registered by the thermometer could be made either over or under zero. If the source of heat was made to play upon the top of the tube, then the temperature would read over zero say 6°, and if made to play on the bottom it would read − 8°, the receiver being − 12°. When however the ice was made to lie on the upper side of the thermometer by turning the latter round, the temperature could not be raised over zero, and sometimes not over − 4°. These experiments were repeated by exhausting with a Sprengel pump, and it was invariably found that the pressure of the gas or vapour in the receiver determined the temperature of volatilisation of the ice, and when the "vacuum" contained only



water vapour the temperatures of the receiver and of the ice round the thermometer (however far apart they were placed) were practically the same. For instance, let the receiver be − 5°, then the thermometer in the ice is also − 5° or − 4°; now let the receiver be suddenly cooled to − 14° while the flame round the ice is urged to a higher temperature; the ice will nevertheless fall to − 13° or thereby; in short, the *temperature* of the "boiling" ice is determined by that of the receiver, while the *rate* of its "boiling" is determined by the temperature of the tube surrounding it. The ice remains perfectly dry, but if air be admitted or the receiver be raised above 0°, melting takes place.

As it has been objected that the thermometer might yield anomalous readings under such conditions (though why I cannot see), another method was tried, as shown at E. A small bulb blown on the end of a tube open at the other end, and containing a little water, had ice frozen round it, as in the case of the thermometer, and was then placed in the flask as before, so that there was a piece of ice under ordinary atmospheric conditions inclosed in the ice *in vacuo*. The tube round the outer ice was now raised to the softening point, but the ice in the bulb did not melt, and continued solid till the bulb was denuded of external ice by evaporation, showing that the ice *in vacuo* was never over 0°. It appears then that ice cannot be raised above 0° under any circumstances, and that the pressure determines the volatilising or "boiling" points of both solids and liquids, as Regnault's work would lead us to suppose.

J. B. HANNAY

Private Laboratory, Sword Street, Glasgow

BEING a reader of NATURE, I have become quite interested in Mr. Thos. Carnelley's experiments with hot ice. Although Mr. Carnelley's experiments would seem to be sufficiently accurate to prove that the ice was in a heated condition, I would still like to offer an additional method to heat the ice, and also a method to test for heat in the ice. To heat the ice I would suggest a small coil of fine platinum wire placed in position in the tube where the water is to be frozen, and the two ends of the coil passed through the sides of the tube and hermetically sealed.

If now the water be frozen around the coil, and a current of electricity passed through the wire of sufficient amount to heat the wire as much as might be determined upon, and the ice yet remain frozen, there would seem to be no doubt about the ice having become heated by contact with the hot platinum wire.

The method I would suggest to test for heat in the ice would be to take a couple of pieces of heavy platinum wire and pass through the sides of the tube and hermetically sealed as before, except to have a small space between the two ends of the wire on the inside of the tube, of one-eighth or one-quarter inch, or as much space as might be thought best.

If now the water be frozen between the ends or all round the ends of the wire, and a small battery and galvanometer be put in circuit with the terminals of the platinum wire, and a gas jet be applied to heat the ice, if the ice becomes heated the

galvanometer should show a stronger current of electricity passing, on the principle that most, if not all, non-metallic substances that are conductors of electricity become better conductors on the application of heat. I judge that the galvanometer test would be a very perfect one. GEORGE B. RICHMOND

Lansing, Michigan, U.S.A., March 5

The Oldest Fossil Insects

I SHALL be glad if you will afford me an opportunity of explaining one or two personal matters referred to in p. 11 of Mr. Scudder's memoir on the Devonian Insects of New Brunswick, which was mentioned in your last number (pp. 483, 4). He very justly takes exception to some bibliographical and orthographical errors committed by me in *Trans.* Entom. Soc. Lond. 1871, pp. 38-40, in a notice of fossil insects named and described by him, and naturally regards them as evidence of insufficient study of the literature relating to them. It is difficult to say precisely what happened upwards of ten years ago, but I am satisfied that the mistakes must have arisen in one or the other of these two ways. Either I attributed the authorship of the names to the person who first published figures of the fossils, on the ground that names bestowed upon insect-fossils by the publication of descriptions, without accompanying figures, rank as mere "Catalogue" or MS. names devoid of priority; or else they are due to circumstances under which the citations were collated. Closely pressed for time, and without much experience in the art of citation, it is as likely as not that, after forming an opinion upon the plates and consulting the letterpress to see what the author had to say about them, I referred from force of habit to the title-page of the volume for the date of publication and the author's name, instead of turning to the heading of the article for this last.

In the same page of his memoir Mr. Scudder alludes to the following passages in p. 39 of my work, over which we had some fun when he was last in England, though the strictures were not aimed at him more than at the others. "Palæontologists have adopted a ridiculous course with regard to some insect fossils. Whenever an obscure fragment of a well-reticulate insect's wing is found in a rock, a genus is straightway set up, and the fossil named as a new *species*. The species is then referred to the *Ephemeridæ*, and is immediately pronounced to be a synthetic type of insects at present distantly related to one another in organisation. This enunciation of synthetic types is often nothing less than a resort to random conjecture respecting the affinities of animals which the writer is at a loss to classify. An insect allied to the *Ephemeridæ* which chirped like a locust (such as *Xenoneura* is imagined to have been), is a tolerable sample of these synthetic types. When a fossil comprises only a fragment, or even a complete wing of an Ephemerid, it is hardly possible to determine the *genus*, and impossible to assert the *species*. The utmost that can be learned from such a specimen is the approximate relations of the insect. Neuration by itself is not sufficient to define the species or even the genera of recent *Ephemeridæ*." What I meant to be deduced from this was that, where in the nature of things actual precision is unattainable, palæontologists should be content to learn and state the "approximate relations" of fossil insects, and not presume upon the ignorance of scientific men in the matter of *genera* and *species*. And I further thought that the *Ephemeridæ* had served quite long enough as an asylum for fossil cripples; I wished to intimate gently that refuse of other groups of insects should be henceforth "shot" elsewhere.

Mr. Scudder does not know by whom the Devonian insects "have all been regarded as allies of the Ephemeridæ." My authority for stating such to have been the case is Sir John Lubbock's Presidential Address in *Trans.* Ent. Soc. London, v.; *Proc.* cxxviii. (1868), where "*Haplophlebium Barnesii* . . . is referred to the Ephemerina," and likewise "*Platephemera antiqua, Homothetus fossilis, Lithentomon Hartii*, and *Xenoneura antiquorum*" are said to be "all Neuropterous and allied to the Ephemeridæ." As members of this family they are quoted by Marschall. *Dyscritus vetustus* was not cited by Sir John; but since Mr. Scudder now states it (p. 22) to be "most closely allied" to Homothetus, there was no harm done in classing it with the rest.

The reason why I thought, prior to the publication of Dr. Hagen's letter in NATURE, that Platephemera might have been an Ephemeron, was that in some respects Mr. Scudder's figure presents an appreciable likeness to the neuration of the fore-wing in species of Palingenia, of which I possess unpublished drawings; but these certainly are not quite so odonatous in detail as Platephemera. Without inspecting actual specimens, it is hazardous to pronounce an opinion about fossils.

A. E. EATON

Chepstow Road, Croydon, S.W., March 28

Oceanic Phenomenon

FROM the description given by Dr. Coppinger of the "confervoid alga" observed on board H.M.S *Alert* some 200 miles to the southward of Tongatabu (NATURE, vol. xxiii. p. 482), the conferva in question would appear to be of a species similar to that from which the Red Sea is said to obtain its name. Whilst proceeding up the Red Sea in H.M.S. *Hornet* during the month of June of last year, I had many opportunities of observing the dirty-reddish scum on its surface—a phenomenon which must be familiar to all navigators of this sea. Each of the little bundles composing it measured about $\frac{1}{25}$th of an inch in length and $\frac{1}{200}$th in breadth, and contained from twenty to fifty filaments, each filament being composed of a linear series of short cells, and measuring $\frac{1}{2000}$th of an inch in breadth. I did not observe the discoid bodies referred to by Dr. Coppinger, but their absence may be explained by assigning to this conferva a particular season for the production of these bodies. Scattered among the bundles were tiny spherical bodies possessing a bristly appearance, which proved to be formed of a confused network of the filaments that composed the bundles.

This conferva would appear to have a very wide distribution. It was observed by Mr. Darwin near the Abrothos Islets which lie off the east coast of South America; and it is with regard to this phenomenon that the author of the "Journal of the *Beagle*" thus writes:—"Mr. Berkeley informs me that they are the same species (*Trichodesmium erythræum*) with that found over large spaces in the Red Sea, and whence its name of Red Sea is derived. In almost every long voyage some account is given of these confervæ. They appear especially common in the sea near Australia; and off Cape Leeuwin I found an allied but smaller and apparently different species. Capt. Cook in his third voyage remarks that the sailors gave to this appearance the name of sea sawdust." H. B. GUPPY

17, Woodlane, Falmouth, March 28

The Banks of the Yang-tse at Hankow

AT the end of January, 1878, when the waters of the Yang-tse occupied their lowest level, I had the opportunity of examining the left bank of the river immediately below the foreign settlement. The bank, which varied from thirty to thirty-five feet in height, did not present a single perpendicular face, but was cut up into two or more terraces formed by the lingering of the waters at those levels for some extent of time. A calcareous loam, homogeneous in appearance and dark in colour, composed the entire bank with the exception of the upper portion, where a layer of sand a few inches in thickness separated two layers of laminated loam, each of them of similar thickness. After a little trouble I was enabled to observe that the apparently homogeneous loam was made up of fine horizontal layers varying from one-thirtieth to one-tenth of an inch in thickness; but the lamination was often concealed; and it was only where the loam had been freshly broken away that the layers were sufficiently distinct to be counted. Shells were embedded in the loam, but mostly in the lower half of the bank; those of the genus "Paludina" were the most abundant, whilst bivalves of the genus "Corbicula" occurred, but not in any numbers. The upper three feet of the river-bank were riddled with the burrows of annelids, and these burrows were often filled with little rounded masses of loam, evidently the excrementitious droppings of the worms.

If, as in the case of the alluvial valley of the Nile, it be considered that each of the fine layers which compose the bank of the Yang-tse was deposited during the periodic annual inundation of the river, then every layer will represent a year's deposit; and taking the average thickness of each layer to be one-twentieth of an inch, it would require twenty years to form an inch and a century to form five inches; whilst the whole thickness as exposed in the river-bank would require for its formation a period of between 7000 and 8000 years.[1]

[1] The borings and excavations round the pedestal of the statue of Rameses at Memphis enabled Mr. Horner to estimate the rate of deposition of the alluvium of the Nile at $3\frac{1}{2}$ inches in a century. (*Vide* Lyell's "Principles of Geology.")

It may be pertinent to the subject of this paper to remark on the general appearance of the region around Hankow. A vast alluvial plain extends to the horizon in all directions; whilst dotted over its surface are several shallow lakes, which are lost in the general flood of waters when the Yang-tse overflows its banks in the summer months. Rising abruptly out of this alluvial formation are a few isolated groups of low hills, which in the time of flood stand out like islands from the surrounding waste of waters.

It would be interesting to asertain whether the banks of the Yang tse possess this lamination whenever the river winds its way through an alluvial plain. I noticed the same appearance in the low banks of the estuary near the village of Wusung; the horizontal layers varying in this instance from one-tenth to one-fourteenth of an inch in thickness. Shells of both fresh-water and salt-water genera—"Paludina" and "Mactra"—were embedded in the bank.

H. B. GUPPY

An Experiment on Inherited Memory

WHEN I was a boy I had an electrical machine and Leyden jar; there was also a dog in the family. As a matter of course I "electrified" the dog, and ever afterwards during the remainder of his natural life he ran away in extreme terror when a bottle was presented to him.

The recollection of this has recently suggested an experiment that may be made by some of the readers of NATURE. By means of a small Leyden jar moderately charged startle *both the father and the mother* of an intended forthcoming generation of puppies. When these are full grown and away from their parents observe whether they are at all disturbed by the sight of a bottle or a Leyden jar, care being taken that the bottle is never shown to the parents in the presence of the offspring.

A single experiment will not be sufficient. It should be tried by several; for which reason I suggest it here. There is no more cruelty involved than in an ordinary practical joke. It is not the pain of the shock, but its startling mystery that frightens the animal, especially if the shock is given by placing the jar on a piece of tinfoil or sheet metal, and allowing the dog spontaneously to investigate by smelling the knob of the jar while his fore-feet are in communication with the outer coating. Under ordinary circumstances the dog obtains through his nose much information concerning the properties of things before he actually touches them, but in this case his whole life experience is contradicted by the mysterious, inodorous, diabolical vitality of the vitreous fiend. A bottle thenceforth makes upon the intellect of the dog a similar impression to that which a sheeted broomstick in a churchyard makes upon the similar intellect of a superstitious rustic.

W. MATTIEU WILLIAMS

Stonebridge Park, Willesden

Meteors

THREE very bright meteors were observed here during the month of December, 1880, and are, I think, worthy of record.

1. December 2, 1h. 14m. 50s. a.m. A meteor brighter than Jupiter descended towards the west point of the horizon, passing about 1° N. of Saturn, and somewhat farther from Jupiter, and in a line therefore nearly parallel to that joining those two planets. The train was visible about three seconds.

2. December 8, 10h. 55m. 30s. p.m. A meteor as bright as Jupiter descended towards the north point of the horizon, about 1° below η Ursæ Majoris, its path being inclined at an angle of about 35° to the horizon. The train was brilliant, but vanished speedily.

3. December 24, 10h. 4m. p.m. A very bright meteor, seen through (or below) the clouds in the south-south-east, shot down towards the south-south-west point of the horizon, at an angle of about 30°. No stars were visible in that part of the heavens at the time.

J. PARNELL

Upper Clapton, March 17

Classification of the Indo-Chinese and Oceanic Races

IN your issue of December 20 (p. 199), just to hand (February 12), I notice a contribution by Mr. A. H. Keane on the classification of the Indo-Chinese and Oceanic races.

As the *Orang Semang* of the Malay Peninsula is only just referred to, I conclude that the author has not seen Maclay's papers on the wild tribes of the Malayan Peninsula in the second number of the *Journal* of the Straits Branch of the Royal Asiatic Society and a memoir by the same writer in the *Journal of Eastern Asia*, of which unfortunately only one number appeared. On the Jakuns, Maclay, who has probably seen more of their inner life and habits than any other ethnologist, writes as follows of the *Semang* and *Lakai* tribes:—"Logan" (*Journal of the Indian Archipelago*, vol. vii. p. 31, 32), "though differing from some others, says that the *Orang Semang* are certainly *Negritos*, but he calls them a mixed race. According to my experience I must declare this also to be incorrect.

"From my own experience and observations I have come to the conclusion that the *Orang Lakai* and the *Orang Semang* are tribes of the same stock, that further, in their physical *habitus* and in respect of language they are closely connected with each other, and represent a pure unmixed branch of the Melanesian race; anthropologically therefore they absolutely differ from the Malays. The Melanesian tribes of the Malay Peninsula, chiefly because of the form of their skull, which has a tendency to be brachycephalic, approach the *Negritos* of the Philippines, and, like the latter, they do not differ very widely from the Papuan tribes of New Guinea."

In the fifth number of the *Journal* of the Straits Branch of the R.A.S., Mr. Swettenham, the Assistant Colonial Secretary for the Native States S.S., thus describes the *Semangs* of Ijoh:—

"These people are short in stature, dark in colour, and their hair is close and woolly like that of negroes, with this difference, that all the men wear four or five short tufts or corkscrews of hair growing on the back of their heads, called *jamûl*."

During my botanical excursion through Perak in 1877 I had two *Semangs* as guides, answering to Mr. Swettenham's description.

The Straits Branch of the R.A.S. is as yet in its infancy, having been established only in 1877, and its *Journal* has probably not yet secured a very wide circulation, although the five numbers that have been published contain probably more authentic information about the Malayan Peninsula than can be found elsewhere.

The characters Mr. Keane has employed to indicate the word "papûwah" are certainly not Malayan; at any rate it would be a matter of impossibility to secure the services of a Malay in Singapore who would be capable of deciphering them. The word, which is a corruption of the Malayan or Javanese adjective *puwah-puwah*, is usually spelt thus— ڤوڤوه

Writing about New Guinea, Crawfurd ("A Descriptive Dictionary of the Indian Islands," p. 300) thus expresses himself about the word Papua:—"Some recent geographers have thought proper to give the great island the name of Papua, but an innovation which is correct neither in sound, sense nor orthography seems to possess no advantage over one which it has borne now for nearly three centuries and a half."

It may not be out of place here to remark that Messrs. Trübner and Co. are the London agents of the Straits R.A.S.

Singapore S.S., February 12

H. J. MURTON

Fascination

IN the interior of the province Valdivia, South Chili, a species of wood-snipe (*Paipayen inc.*) is often caught by the natives in the following manner:—When the bird flies into one of the low bushes, which in spots of about three to six metres diameter are found frequently in the wood-meadows there, two men on horseback go round it in the same direction, swinging their lazos over the bush. After ten or more rounds one man slips down from his horse, whilst the other continues, leading his companion's horse behind. Carefully then the first man creeps on to the point, where the paipayen is sitting nearly motionless or stupefied with the rider's circular movements, and kills it by a quick blow of a stick.

When I first was told so I would not believe it; but in 1853 or 1854 I took part myself in this kind of capture in the hacienda San Juan, in Valdivia, belonging to my chief, Dr. Philippi, now professor in the University and director of the museum in Santiago. I had left the house without gun, accompanied by a native servant, when, in a part of the wood called Quemas, I observed a paipayen falling into a dense but low bush of the

above-mentioned kind. Desiring to obtain a good specimen of this not very common bird for our collection, I expressed my regret at not having the gun, but the servant replied: "Never mind, if you wish, we will get the bird." And he caught it with my assistance in the above way without injuring it.

Marburg, March 16 CARL OCHSENIUS

Flying-Fish

JUNE 11, 1873, at sea 300 miles south of Panama, I saw a man-of-war hawk and a school of bonitos in pursuit of a school of flying-fish. As one of the latter came out of the water, closely pursued by his enemy, the hawk swooped down, not fifty yards from the ship, but missed his prey, the fish *apparently* turning from its course to avoid him. A second attempt was more successful, and the hawk flew off with the flying-fish in his talons. The whole affair was plainly seen, as also was the continued chase of the flying-fish by the bonitos.

ALLAN D. BROUN,
Commander U.S. Navy

U.S. Torpedo Station, Newport, R.I., U.S.A., March 10

THE OXFORD COMMISSIONERS ON PROFESSORS

WE are not disposed to agree with the outcry which has been raised in some quarters in reference to the proposition of the Oxford University Commissioners to enact certain regulations with the view of compelling Oxford Professors to reside in the University and to give lectures.

Some of the Commissioners' regulations relating to this subject appear to many to be ill-advised, but they have been improved by the recent modifications, and the general intention seems not only a right one, but also one which must be carried out whenever public opinion is brought to bear on the matter.

A very simple view of the matter may be suggested. The professors in the English Universities might be put on the same footing as are the professors in German Universities. In those Universities the professors carry on abundant research; they also give very numerous lectures, usually what may be called "representative courses," that is, courses in which an attempt is made to present to the student the main outlines and much of the detail of the subject professed. Even in the Collège de France at Paris, which is *not* (strictly speaking) an educational institution, each professor is required to give an annual course of lectures (to the number of forty, we believe).

Research and the advancement of learning are, we do not for a moment doubt, the highest, and therefore in a certain sense the first business of University professors. It is perhaps because this is so generally admitted that the Commissioners did not at first insist upon it. But it is in order that he may teach—not huge popular audiences nor cram-classes, but devoted thoroughgoing students—that the professor creates new knowledge. His best result is not new knowledge itself, but new youthful investigators ready and able to carry on the researches which he has commenced, and through which they have learnt method and gained enthusiasm. There is no stimulus to research so healthy and so sure as that afforded by the opportunity of converting a class of generous-minded young men into ardent disciples and loving fellow-workers.

Hence, it may be maintained, there is no necessary antagonism between *true professorial teaching* (*i.e.* definite courses of lectures) and the profoundest study and research.

That the Commissioners have introduced no binding regulations with the object of forcing a professor to carry on research, is, we believe, a proof of wisdom and a just tribute to the dignity of such work. No regulations can make an investigator: the question as to whether a given professorship will be used for the advancement of science and learning is decided before any regulations can have effect, viz., when the choice of a person to fill the post is made. If he is a "searcher" already, he will remain so; if he is not, a bad choice will have been made, and no regulations as to research can ever amend it. It is, however, well that the Commissioners have seen fit to improve their first set of regulations in so far as to state that an Oxford professor is *expected* to advance the study of the subject to which his chair is assigned.

The measures which the Commissioners propose for insuring the delivery of lectures by Oxford professors are objectionable on the ground that they are purely penal. They should be persuasive. The German professor is only too glad to give a thorough and attractive course of lectures if he has it in him to do so, because he thereby doubles or trebles the income which he derives from endowment. The Oxford Commissioners have made a great mistake in prohibiting the professors from charging fees for the compulsory course of two or three lectures a week. All students, whether belonging to the professor's own college or not, should be liable to pay fees to the professors for attendance on their courses of instruction, whether lectures or laboratorial. It is only by so arranging the position and endowment of a professor that he is both able and willing to increase his income by the fees paid by his class, that a really firm and satisfactory basis for the regulation of a professor's duties can be obtained.

It has been maintained that where an income derived from an endowment of 600*l*. can be increased to 1000*l*. a year by the receipts from lecture-fees, the professor will be anxious to give such lectures as will attract students—and in spite of objections ready to hand, it is held that those are the lectures which should be given. It is not true that a professor so circumstanced will necessarily degenerate into a mere examination coach. If he should be tempted to do so the fault lies with the examination. The professor should himself have a voice in the arrangement of the examination, and care should be taken by the University that it is so organised and defined in all its parts that students who have carefully followed a high class of professorial teaching, such as would be offered by a Huxley, a Ludwig, a Bunsen, or a Fischer, should come to the front in it rather than those who have crammed with some newly-fledged classman, or with an experienced "coach" versed in all the artifices of sham knowledge.

It appears to be an excellent and necessary provision to which it is to be hoped that the Commissioners will adhere in spite of all opposition, that the professors in each faculty should with other University teachers in the same faculty constitute a council having the power of controlling to some extent the lectures of each individual professor. There is no degradation in this; it is the almost universal custom in existing Universities. The faculty has to provide for the teaching of its proper studies, and naturally must exercise a friendly control over the extent and scope of the courses of instruction offered by its members.

It is owing to the absence of any such control at the present moment that even by those Oxford professors who do lecture, no representative course on *any subject* is ever given. A student in Oxford cannot by any possibility attend a thorough *course* of lectures or laboratory instruction in physiology, nor in zoology, nor in botany, nor in physics, nor in chemistry. And yet in the smallest as well as the largest of the often despised "medical schools" of London, a student has provided for him courses of from thirty to a hundred lectures every year in all these subjects, as well as in others, to be attended, of course, in successive sessions. The same absence of complete or representative courses of instruction is to be noted at Oxford in other departments, such as philology, archæology, various departments of history, &c.

The sole cause of the existence of such complete courses in other institutions than Oxford—over and above the primary one connected with the income from fees—is that the professor has to submit his scheme of lectures for the ensuing session or year in a general way to his colleagues, who would suggest to him a more complete or more representative program, were his proposals considered insufficient, and might take steps to supplement his teaching by the appointment of a supplementary professor (thus diminishing the original professor's income from fees), were he to prove intractable.

The keystone of the professorial system, on which all such control and persuasion, co-operation and reciprocal criticism, must rest, is the income from class-fees. In having not only not insisted upon this, but in having actually prohibited the free levying of fees, the Oxford Commissioners have made their scheme for professors absolutely unworkable. They have simply played into the hands of those who have at present a most injurious monopoly of the fees paid by students, and who give in return as little and as inadequate teaching as they please, namely, the confederacy of boarding-house keepers known as "college tutors and lecturers."

The proposal that professors should examine their classes and report to the Heads of Colleges as to the performance of each student was characterised by a spirit of petty interference quite unworthy of the large objects placed before the Commissioners, and has very properly been withdrawn. Such details, together with some other points, might well have been left by the Commissioners to the Councils of Faculties, which they so wisely intend to bring into existence.

It may be urged that if the Commissioners were to confine themselves in these and similar matters to creating the organisation which is terribly needed at Oxford, and of which these Councils of Faculties promise to be the most powerful and important part, they might with very great advantage leave the question of terminal examinations, and the scale of fees to be charged for lectures, &c., to be worked out by the reorganised University itself. But instead of prohibiting class-fees they should have strengthened the hands of the professoriate in the competition with the powerful band who are interested in maintaining the disastrous and absurd system of college tuition and tuition-fees. So long as the undergraduate is forced to pay to college tutors a lump sum of 25*l.* a year, he will seek his instruction (whether he finds it or not) from those whom he has been compelled to pay, and not from the professors whom he is not allowed to pay.

It is clear that with the present body of free-holders it was necessary for the Commissioners to insist on the new principle that a professor is not to be free from responsibility (*Lehrfreiheit*, we may observe, does *not* mean "freedom *from* teaching," as some writers who in the daily papers have recently appealed to German precedents almost seem to fancy), but is, on the contrary, to be charged with certain duties and to be responsible in a measure to his brother professors for performing those duties in a satisfactory manner.

ACADEMICUS

THE INTERNATIONAL GEOLOGICAL CONGRESS

THIS Congress is to hold its second session at Bologna, commencing on September 29, 1881, under the presidency of Signor Q. Sella, president of the Accademia dei Lincei of Rome, and under the patronage of His Majesty the King of Italy, who has liberally placed the necessary funds at the disposal of the Italian Committee of Organisation, of which Prof. J. Capellini of the Bologna Museum is the president, and General Tarannelli of the University of Pavia the secretary.

The movement sprang out of a suggestion made at a meeting of the American Association of Science held at Buffalo, New York, August 25, 1876, that an International Geological Congress was advisable, to insure uniformity of methods of representing geological phenomena, and the value of terms. Towards this end a committee of organisation was formed, of which Prof. James Hall was president and Dr. Sterry Hunt secretary, in which England was represented by Prof. Huxley, and Sweden by Dr. Otto Torell. The result of their deliberations was the first session of the Congress held at Paris, in the Palace of the Trocadéro, under the presidency of Prof. Hébert and the patronage of the Minister of Public Instruction. At the Congress, which lasted six days, two International Commissions were appointed, the one to consider geological cartography, with a view of adopting a common system of signs and colours, the other to investigate the possibility of effecting the unification of geological nomenclature and to consider all matters relating to stratigraphical classification and nomenclature, to a certain extent involving an inquiry into the value and significance of petrological and palæontological characters. A third Commission, entirely French, was also appointed to report on Bologna, on the rules to be followed in establishing the nomenclature of species in mineralogy and palæontology.

M. Renevier, general secretary of the first Commission, has just published his second report of progress, and states that advantage was taken of the presence of several members of the Commission during the fiftieth anniversary meeting of the Geological Society of France on April 2, 1880, to hold a meeting of the Commission at which five European countries were represented, under the presidency of M. Daubrée; since then, more or less detailed reports from nearly all the committees representing different countries have been received, except from Canada, presided over by Mr. Selwyn, and Great Britain by Prof. Ramsay. In some of these schemes there is a considerable amount of agreement. Quaternary deposits being represented by a pale green, Pliocene by pale yellow, Miocene dark yellow or orange, Eocene by bistre, Cretaceous by green, Jurassic by blue, Lias by violet, Trias by burnt sienna, Permian and Carboniferous by dark grey, Devonian by brown, or brown stripes on pink, Crystalline schists by rose carmine, Granite by dark carmine, divisions in the various rocks being expressed by tints of the same colour, or by shading or dotting.

The General Secretary of the Commission for the Unification of Nomenclature is M. Devalque, who reports that this Commission also met at the Paris Geological Society's anniversary, France being represented by M. Hébert, Switzerland by Prof. A. Favre, and Great Britain by Prof. Hughes. The latter Commissioner, aided by Prof. Prestwich, has now succeeded in organising a British sub-Commission, who have appointed six committees to inquire into groups of formations, and (1) to draw up a list of the names now in use; (2) to ascertain the true significance of such names or terms, giving reference to the authors by whom they were used in the first instance, or subsequently with a modified meaning; (3) to investigate into the synonomy of such names and terms in the first place as regards the British Isles, and afterwards to inquire into their correlation with them in use in other areas; and (4) to offer suggestions for the unification of the nomenclature. As the committees can seldom sit, as their members are scattered, they have been modelled on the principle of the Inquiry Committees of the British Association, and have attached to them one or two "reporters," charged with assimilating the views and facts collected by the Committee. The reporters for the British Isles, are for Recent and Tertiary rocks, Messrs. Starkie Gardner and H. B. Woodward; for Cretaceous rocks Messrs. Topley and Jukes-Browne; for Jurassic rocks Messrs. Huddlestone and Blake; for Trias and Permian, Mr. De Rance and the Rev. A. Irving; for

Carboniferous, Devonian, and Old Red, Messrs. Morton and Strahan; for Silurian, Cambrian, and Pre-Cambrian, Messrs. Lapworth and Marr. For chemical, dynamical geology, petrology, and mineral veins Messrs. Bauerman and T. Davies.

The last-mentioned committee is specially to consider the question of nomenclature under the following general heads: (1) Terms founded on physical characters; (2) founded on mineral composition; (3) founded on names of places; (4) founded on local peculiarities and common usage; (5) founded on theories of origin and other hypotheses; (6) synonyms; (7) suggestions for systematising and for unification of nomenclature.

The Sub-commission or General Committee has Prof. Hughes for its chairman, and Mr. E. B. Tawney for its secretary; its duty is to receive the reports of the Committees and to consider the value of terms. The list of names forming the Sub-Commission includes those of Mr. Etheridge, P.G.S., Professors Bonney, Boyd Dawkins, Haughton, Hull, Judd, Lebour, Morris, Prestwich, Rupert Jones, and Seeley; Doctors Clement Le Neve Foster, Evans, Geikie, J. Geikie, Hicks, Nicholson, and Sorby, and the names already mentioned, of members acting as Reporters, Secretary, and the Chairman. The Sub-Commission consider that the word *system* should be used as the term indicating the largest sub-division, applied to a group which stands by itself, easily and clearly distinguishable from the rocks above and the rocks below, bounded above and below by triads in stratigraphical regions, and characterised by special forms of life. *Formation* expresses a smaller group, with some lithological and palæontological characters in common, but which may be in continuous sequence with the rocks above and below. *Deposit* implies similarity of lithological character. *Layers, laminæ, bed, group, series,* and *rock* are still under discussion. *Zone* and *horizon* were defined; but *cycle* and *data* were left open questions.

Through the liberality of His Majesty the King of Italy, the committee of organisation are able to offer a prize of 5000 francs for the best suggestion for an international scale of colours and conventional signs practically applicable to geological maps and sections, including those of small scale. The index of colours and signs should be accompanied by maps representing regions of varied geological structure, and by an explanatory memoir in the French language. The documents should be marked with a motto, which should be placed on the outside of an envelope containing the name of the author, which will not be opened until the Congress, when the name of the successful competitor will be made known. The index and accompanying papers should be sent in to Prof. J. Capellini, director of the Museum at Bologna, by the end of May. The award will be made by a jury of five chosen from the presidents of sub-commissions. Should no index be thought worthy of the grand prize, the best will receive a gold medal of the value of 1000 francs, while to the two next will be given medals of silver and bronze of similar shape. C. E. DE RANCE

THE FALLS OF NIAGARA IN WINTER

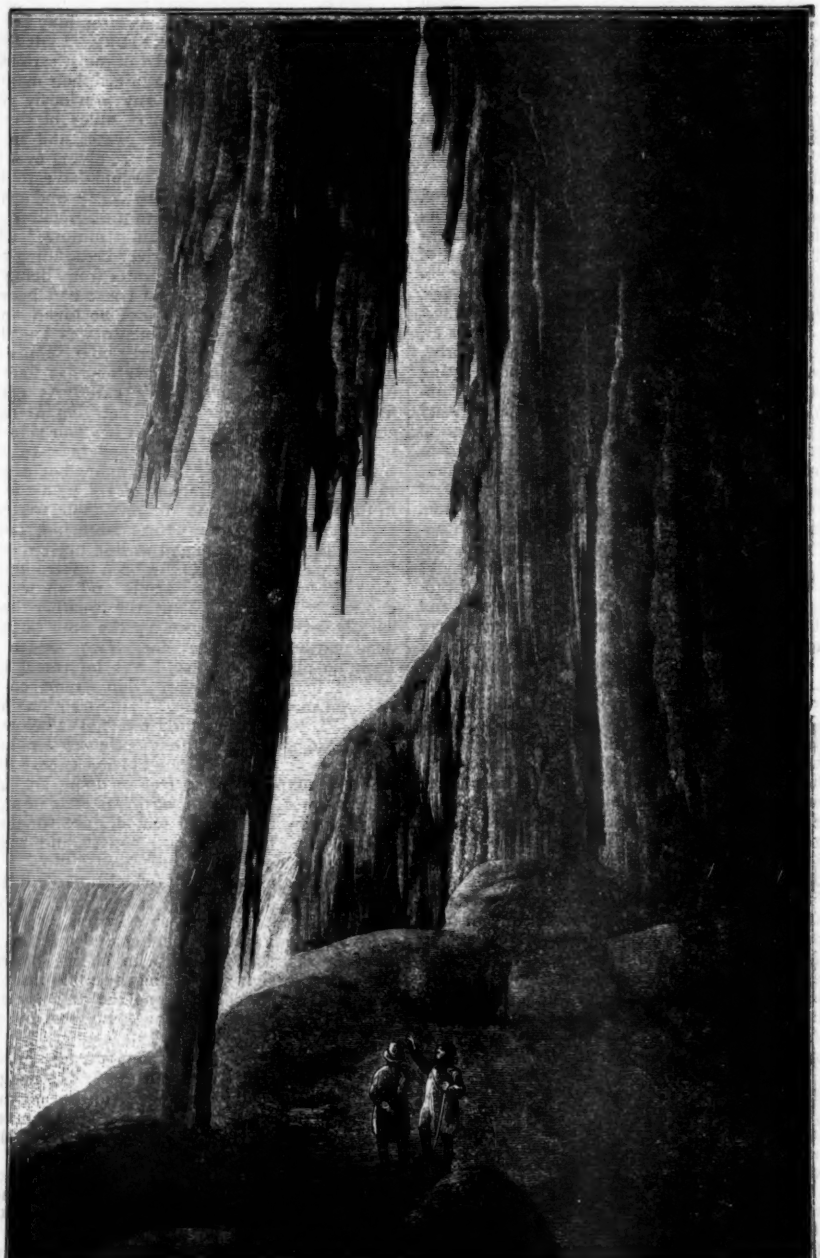
IN the first week of last February it fell to my lot to make very hurriedly the transcontinental journey of 3500 miles from San Francisco to New York. Before starting I resolved that the one stoppage which I could allow myself *en route* should be made at Niagara. I had visited the Falls in the early summer of 1879, and was so profoundly impressed by them that I could not resist the opportunity of seeing them again under their wintry aspect; and I was confirmed in my resolve by seeing statements in various American papers to the effect that, owing to the long-continued and exceptionally severe cold of the present winter, the Ice-mountains at the Falls were higher than had ever been previously known. These statements were confirmed to me on the spot by several persons long resident in the village.

Two or three preliminary notes on the journey across the Rocky Mountains in midwinter may not be without interest for the readers of NATURE. I left San Francisco on February 2nd in the midst of most serious floods, and on that particular day they attained their maximum, which was one inch higher than any previously recorded. It was estimated that 3500 square miles of the most fertile land of California was under water, and in many parts steamboats of light draught were plying over the country. Any assessment of damage would have to be made by millions of dollars. I heard many and grievous complaints of the damage done to the agricultural interests of the country by the "hydraulic mining," which washed the hillsides down into the river beds, filling them up, and thus prevented much flood-water from being carried off. In some places the railroad track had been apparently washed away, for it could not be found, and from this cause our journey to Sacramento was lengthened about fifty miles, as the gigantic ferry-boat *Solano* could not be used for the short route. This boat has four tracks upon it, and will carry twenty-four cars. As each car seats fifty people, this is equal to carrying a train that will accommodate 1200 people. It has four side-wheels, each with its engine and set of boilers. In crossing the Sierras we encountered little snow, but a great deal of rain. The greatest amount of snow on the journey was in the upper part of the Weber Cañon, 100 miles east of Ogden and Salt Lake. Here there had been considerable difficulty in keeping the line open during January, but the train-service had not been interrupted for a single day, although the snow-sheds and snow-ploughs were constantly required. That the weather had been unusually severe was shown by the very large number of dead cattle along the line, from Ogden across the Laramie plains, and also, I was informed, in Colorado. In the four days between San Francisco and Omaha (where we arrived punctually), the terminus of the Pacific Railroad, the temperature was never below 26° F., and the air so still that I frequently saw smoke rings from the locomotive funnel expand to 6 or even 8 feet diameter, rising perhaps 30 or 40 feet in doing so. All the cars were warmed, usually to too great an extent, from 70° to 75° F., being the normal temperature for the interior of railway cars, hotels, private houses, and schools, as far as my experience went.

East of the Missouri (which, like all the rivers I crossed, was frozen over) trains were everywhere very much delayed, owing to snowstorms, or to the slippery state of the rails, which were coated with ice. The utmost caution was used by those in charge of trains, and a strong impression was left on my mind that safety, and not speed or punctuality, was the primary consideration in such American railway management as I came across.

On leaving Chicago a phenomenon presented itself which is common enough in America, though but rarely seen in this country, and never on so gigantic a scale. For several days the temperature had been very low, and every object was exceedingly cold. On the night of February 6th, the air-temperature rose to 33° F., and fine rain fell. This froze upon everything and encased it with transparent ice, from which in many instances delicate icicles depended. Sad havoc was played with the overhead telegraph wires in Chicago itself (which were broken by the weight); but on leaving the city in the early morning the exceeding beauty of the whole country, usually so uninteresting from its flatness, became apparent. A light coating of snow lay on the ground, but everything, every twig, every dead leaf, every blade of grass, had its own transparent covering, which in the occasional gleams of the sun shone with the most gorgeous colours.

For seven or eight hours we travelled through this, a distance of some 250 miles, and I heard of similar and simultaneous appearances over larger tracts of country.



Gigantic icicle under Table Rock, photographed in January, 1881. The upper right-hand corner is rock, and a portion of the Canadian (or Horse-Shoe) Fall is seen on the left. The whole of the apparent ground is a mass of frozen spray which has accumulated many feet in thickness on the shingle, &c., at the foot of the rock.

Probably the most wonderful exhibition ever seen, of, not frozen rain, but frozen spray, was to be found at the Falls of Niagara. A large number of readers of NATURE have visited them, and possibly all are sufficiently familiar

with their topography, through the medium of books and photographs, to render any general description unnecessary. I will therefore confine myself to the special features produced by this winter's cold.

The whole district lay under a thin coating of snow, and all the roads were in good condition for sleighing, indeed those near the Falls were so completely ice-covered with frozen spray, as to render no other mode of locomotion possible. Those who have seen both places have probably been struck, as I was, with the strong resemblance between the gorge of the Niagara river below the Falls, and the gorge of the Avon at Clifton, Bristol. The latter is the finer of the two, being narrower, and having higher sides, but both are limestone gorges, and similar in character. In the Niagara gorge numerous springs discharge themselves into the chasm at various points in the precipitous rocky sides, and at these points numerous collections of huge and massive icicles appeared as though adherent to the rock, measuring perhaps seventy or eighty feet in length, and eight to ten feet in irregular diameter. In the exquisite purity of their colour and general appearance, they reminded me strongly of the pillars of ice in the upper part of the Rhone glacier.

The width of the river itself was not a little lessened, both in the rapids above and the comparatively still water below the Falls, by the ice at the banks, and it was a matter of surprise to notice how much ice accumulated at the edges of water that was running very rapidly. At the top of the American Fall itself there were so many accumulations of ice that the Fall was actually divided into five separate and distinct Falls, in the same way as, even in summer, that portion of the Fall which is in front of the "Cave of the Winds" is cut off by rocks on the upper edge, from the main body of the Fall.

The mention of the "Cave of the Winds" recalls also that huge boulder, the "Rock of Ages," in front of this portion of the Fall. That however is only one of many others in front of the American Fall, and these boulders are, as it were, gigantic nuclei, round which the frozen spray accumulates, and produces the Ice-mountain of which we hear so much, and the remains of which are not unfrequently to be seen even by summer visitors. The average height of this is about half the total height of the Fall, but this winter it has attained to the unprecedented height of within twenty feet of the top of the Fall! This highest point is at about one-third of the total width of the Fall, measuring from Goat Island. Between the foot of the incline from Prospect Park and the edge of the Fall is another very high mass. The ice approaches very close to the front of the Fall, and the whole basin into which the water descends is thus closely surrounded, and partially covered, with an enormous and irregular mass of pure semi-transparent ice, of (on the day of my visit) the most beautiful emerald green hue! Later in the day I had the good fortune to fall in with Mr. Bradford, the artist who is so well known for his pictures of Greenland scenery, and in discussing the ice cones formed at waterfalls, he mentioned that, having passed a winter in the Yo Semité valley in California, he had seen an ice-cone close to one of the celebrated Falls there, which was at least 600 feet in height.

Within the last few years a considerable portion of "Table Rock" has fallen away. In its present condition a stream of water about one foot in thickness falls over it in summer, and, owing to the amount of its overhanging, it is easy to get between this Fall and the rock, and thus to be "behind Niagara." At the time of my visit (February 8th), however, the whole of this portion of the Fall was *completely frost-bound*. Enormous icicles, of the most surpassing beauty, depended from the rock above, while at my feet were masses of the frozen spray from the Horseshoe Fall. The intense emerald green of the water of that Fall, seen through and between these magnificent ice-pendants, could be reproduced by no artist, but will never be effaced from my memory. The accompanying woodcut, photographed on to the wood block from a photographic picture taken a few days prior to my visit, will, to those who know the place, give some faint idea of the beauty of the scene, and of the gigantic scale of the icicles. It is scarcely necessary to say, perhaps, that the circular wooden staircase by which the descent under Table Rock is effected, was covered with many feet thickness of ice on the side next the Fall. As the air-temperature was slightly above 32° F. and the icicles were occasionally falling around us, my guide was unwilling that I should remain long, or make any attempt to measure any of the ice-masses.

The fourth, and to the casual visitor perhaps the most remarkable effect of the cold in the immediate neighbourhood of the Falls, is the manner in which every surrounding object is coated with an immense thickness of frozen spray. The trees on Goat Island and in Prospect Park are thus covered to a slight extent, and present a very beautiful appearance. The strangest examples, however, occur on the Canadian side, close to the Horseshoe Fall, where huge irregularly-shaped masses of ice are seen, some of which resemble, in general form, merely a colossal bunch of grapes standing erect on its stalk. A little investigation shows that these are trees, staggering under the weight of tons of ice. Not unnaturally they have many broken branches, and have almost invariably lost their tips. In one instance which I saw, and of which I obtained a photograph, the spray had so accumulated in front of the trunk of a tree about nine inches in diameter, that it had formed a wall of ice *five feet* in width, and of the same thickness as the diameter of the tree-trunk. A flagstaff planted on Table Rock had four or five projections from its top, varying from three to five feet long, and looking like "frozen streamers," or as though watery flags had been flying, and had suddenly been frozen. These were so inaccessible and so dangerous to the passer-by, that they were daily shot down with rifle-bullets! The museum with its pagoda and the adjoining houses close to the Horseshoe Fall were cased with sheet-ice and pendant icicles to such an extent that much of the frozen spray had to be removed daily with an axe.

I mounted to the pagoda (well remembered, I have no doubt, by summer tourists) and there I listened to the "Music of Niagara," of which Mr. Eugene Schuyler has given in the February number of *Scribner's Magazine* an account so interesting, that I venture to conclude this article with a short abstract of it.

Mr. Schuyler starts with the statement that "the tone of Niagara was like that of the full tone of a great organ. So literally is this true that I cannot make my meanings clear without a brief outline of the construction of that great instrument." He then explains the mutual relation of the various pipes, the "ground-tone, over-tones or harmonics, and under-tones or sub-harmonics," and relates his experiences in the Cave of the Winds, on Luna Island above the Central Fall, at the Horseshoe Fall among the rapids, and at the Three Sister Islands. "In fact, wherever I was, I *could not* hear anything else! There was no *roar* at all, but the same great diapason—the noblest and completest one on earth!" Further details of visits to various points are given, and it is interesting to notice that although previously unacquainted with the difference in height of the two Falls, Mr. Schuyler unhesitatingly pronounced the Horseshoe Fall to be several feet lower than the other, guided solely by his musical ear. He then proceeds thus:—

"Now, what is this wonderful tone of Niagara? or rather, what are all these complex tones which make up the music of Niagara? With more or less variation of pitch at various points (to be accounted for), here are the notes which I heard everywhere:—

Just these tones, but *four octaves lower!*

"At once it will be incredulously replied, 'No human ear ever has heard, or ever can hear, tones at such a depth.' I arrived at my conclusions both theoretically and practically, and the two results coincided exactly." For the explanation of this, those interested will do well to consult the article itself. It may be noted here, however, that notes 3 and 4 were heard *everywhere;* that the 5th and 6th were perfectly distinct, but of far less power; that the 7th (the interval of the tenth) was of a power and clearness entirely out of proportion to the harmonics as usually heard in the organ, &c.; and that the 8th, 9th, and 10th notes were only heard occasionally and with a transient impression. Mr. Schuyler then points out that, allowing for the fact that the diameter of Niagara is the *greatest* possible compared with its height, the length of an organ-pipe necessary to give the key-note of Niagara (four octaves below note 1 in the diagram) would be just the average height of the Falls! The figures given are 170·66 feet – 10·24 feet = 160·42 feet, where the 10·24 feet is the allowance for the extra diameter of Niagara treated as an organ-pipe.

It appears, then, that the tone of Niagara is, *note for note*, the dominant chord of our natural scale in music. Its rhythm is one note per second, with three notes in each measure, the first note being the accented one, and the single beats are represented by groups of three semiquavers, where M.M. 60 = 𝅗𝅥, or three times three, three times repeated.

Mr. Schuyler thus concludes in words with which I heartily sympathise. "I have spoken only of the pitch and rhythm of Niagara. What is the *quality* of its tone? Divine! There is no other word for a tone made and fashioned by the Infinite God. I repeat, there is no *roar* at all—it is the sublimest music on earth!"

WILLIAM LANT CARPENTER

ZOOLOGICAL RESULTS OF THE VISIT OF PROF. K. MOEBIUS TO MAURITIUS[1]

THIS work, which is illustrated by a map and twenty-two plates, contains the results of the investigations of Prof. Möbius on the marine fauna of Mauritius and the Seychelle Islands, embodying the account of observations made by him on the spot, and of work done on the collections which he brought home with him on his return from his visit to the islands. It commences with an account of the journey to Mauritius in 1874-75; an account of the Suez Canal is given, and of the voyage through the Red Sea, where Trichodesinium, the yellowish-red floating algæ supposed by some to have given the name to the sea, was met with in abundance. After the well-known tanks of Aden and the Somali divers who surround every ship that comes into the port have been described, Réunion is touched at, and at last Mauritius.

A concise account is given of the geographical, geological, and climatic peculiarities of this island, which is about one-third the size of Holstein. The centre of the island is occupied by a plateau elevated over 1700 feet above sea-level, the highest point being 2711 feet in height. The plateau is surrounded on nearly all sides by mountains, and from these on all sides but the northern, where there is a gradual inclination, rivers and streams fall down very steep slopes with frequent waterfalls into the sea. Rains are very heavy, and the mountain torrents swell with remarkable rapidity. The geological structure of the island is entirely volcanic, with the exception of beds of coral rock. The mean temperature of the year is about 25°·85 C. Rain is most abundant from December to May. The prevailing wind is the south-east trade. Cyclones are sometimes experienced in the period, December to April, but do not occur every year.

Mauritius had originally no mammalian inhabitants excepting bats. The great fruit-bat (*Pteropus vulgaris*) is abundant in the woods. These fruit-bats are easily tamed. One of them was a great pet of Mr. G. Clark, now dead, who was the author of "A Brief Notice of the Fauna of the Mauritius," published in the *Mauritius Almanac* for 1859, and containing some very good observations. This tame bat was taken when young from its mother's breast and brought up by hand. It could not fly, because its wing membranes had been cut through to prevent its doing so. It usually passed its time hanging on to the back of a chair. Directly Mr. Clark came into the room it cried out loudly to be nursed. If it were not taken up at once it climbed up to him, rubbed its head against him, and licked his hands. If Mr. Clark sat down the bat hung on at once to the back of the chair, and followed all the movements of its master with its bright eyes. If its master caught hold of a fruit it climbed forth-with down his arm to his hand to get its share, and it always got two teaspoonsful out of every cup of tea or coffee. If Mr. Clark took any kind of object in his hand the bat climbed to it, examined it with its eyes and nose, and only returned to its chair-back after completely satisfying its curiosity. It followed its master even into the open air if the door was not shut to prevent its getting out.

A good many mammals have been introduced into the island, and are now abundant. A monkey from the East Indies (*Macacus cynomolgus*) inhabits the woods, and makes excursions from thence to plunder the sugar-cane fields. One of the species of the curious hedgehog-like insectivora of Madagascar (*Centetes ecaudatus*) was introduced in the island at the end of the last century. The animals live in damp places and lie in a state of sleep (= hybernation) in the dry season, sleeping then so soundly that they do not awake even when dug up. As soon as the rainy season begins in November they wake up and breed, producing three litters of fifteen or sixteen young every year. The young follow the mother, who calls them with a grunting noise, in a row behind, and protects them when molested with her teeth and spines. A full-grown male weighs as much as four pounds. The animals are so abundant that on a moonlight night with trained dogs twenty or thirty may be caught by one hunter. They are eaten by the working classes.

Besides these there is a shrew mouse, also introduced from the East Indies, a small hare, and the ubiquitous common rat, both of which latter gnaw and destroy the sugar-cane. A stag (*Cervus hippelaphas*) introduced by the Portuguese inhabits the woods. It breeds in July and August, and casts its horns in December or January.

We cannot follow the author in his short reference to the birds and account of the fish. The coral-reefs of the island appear to abound with animal life of all kinds. Several of the corals composing them are laid dry constantly at low tide, and remain exposed to the air without injury. *Goniastræa retiformis* and *Leptoria gracilis* are cited as examples of such. Whilst these corals are in this condition, the polyps remain entirely withdrawn, and the whole surface of the coral laid bare is covered with slime, which prevents its drying up.

In the Seychelles, of which a short account is given, the giant turtle (*Chelone virgata*) is kept in ponds as at Ascension, and is caught with a rope round the flipper, and dragged out to be slaughtered when convenient. The

[1] Beiträge zur Meeresfauna der Insel Mauritius und der Seychellen, bearbeitet von K. Möbius, F. Richter und E. von Martens, u.s.w. (Berlin: Otto Enslin. 1880.)

author here dug up a Cæcilian (*Cæcilia virgata*), and amused himself with the curious leaf insect (*Phyllium siccifolium*).

The Introduction to the book is followed by a long paper by Prof. Möbius on the Foraminifera of the Mauritius, illustrated by many finely-executed plates. Amongst other Rhizopods a Haliphysema occurs, the animal which, by a most extraordinary blunder, was made out by Häckel to have a multicellular structure, and supposed to represent a Gastræa of modern times. Prof. Möbius confirms the observations of Carter, Savile Kent, and Ray Lankester, to the effect that the animal is in reality simply a Rhizopod. He has examined the structure of the Foraminiferous shells which he describes, very carefully by means of sections. He does not, however, add anything of importance to our knowledge of the structure of the soft tissues of the group.

An account of the Decapod Crustacea by Dr. F. Richter follows that of the Foraminifera. Two crabs of most extraordinary habits are described in this portion of the work. Both belong to the family Polydectinæ. The crabs of this family have their front claws armed with large teeth. Latreille, who first named the crab *Polydectes cupulifer*, remarked that a gummy substance was always to be found at the ends of the claws of this species, and Dana described the animal as having always something spongy in its hands. Dr. Möbius has discovered the remarkable fact that these things held in the two claws of the crab are in reality living sea-anemonies. These sea-anemonies are attached to the immovable joint of each claw, whilst the teeth of the movable joint of the claw are kept buried deep into the flesh of the sea-anemonies, and thus hold them fast, although each anemony can easily be pulled away from its position with the forceps in specimens preserved in spirits. The mouth of the sea-anemony is always turned away from the crab. The same curious combination exists in the case of another species of the same family but of a different genus, *Melia tessellata*, which also inhabits Mauritius. A figure is given of this crab with its pair of Actinias, named by Möbius *A. prehensa*, with fully expanded tentacles, held out one in each hand. Möbius gives the following account of the matter. "I collected about fifty male and female specimens of *Melia tessellata*; all of these held in each claw an *Actinia prehensa*. The recurved hooks of the inner margins of the claw joints of the crab are particularly well adapted to hold the Actinias fast. I never succeeded in dragging the living Actinias out without injuring them. If I left the fragments of them when pulled out lying in the vessel in which the *Melia* was, the crab collected them again into its clutch in a short time. If I cut the Actinias in pieces with the scissors, I found them all again in the claws of the crab after a few hours. It is very probable that the Actinias aid the crab in catching its prey by means of their thread-cells, and that the Actinias, on the other hand, gain by being carried from place to place by the crab, and thus brought into contact with more animals which can serve as food to them, than they would if stationary. This is a very interesting case of commensalism."

The work closes with a long account of the Mollusca of Mauritius and the Seychelles by Prof. E. von Martens.

H. N. MOSELEY

NOTES

THE centenary of the birth of George Stephenson is not to be allowed to pass by in a fruitless way in Newcastle-upon Tyne. Dinners, speeches, trade-processions, enthusiasm and bunting—all this was to be expected in a place so intimately connected with the birth of railways. But more than this will probably be done, and we are glad to hear that a scheme is on foot for commemorating the 9th of June in a more useful and more lasting manner, viz. by providing a "Stephenson College" for the use of the houseless but hard-working College of Physical Science of the University of Durham in Newcastle.

THE French Association for the Advancement of Science has been in existence only ten years, but in that short time it has met with astonishing success, and has done some excellent work. To the fifteen sections already existing it proposes to add a sixteenth, under the name of the Section of Pedagogy, and a committee of members will discuss its formation at the forthcoming meeting at Algiers. The subjects of which the Association takes cognisance are divided into four groups, viz., Mathematical Sciences, Physical and Chemical Sciences, Natural Sciences, and Economic Sciences. A goodly list of papers has been already announced, among the authors of which we notice some of the most prominent *savans* in France. We trust, however, that the Association will not degenerate into a great excursion organisation, as to some extent it appears to have done this year. Thus the meeting lasts for six days, while the return tickets, issued in connection with the Association under very liberal terms, are good for six weeks, and no less than fifteen excursions in the neighbourhood of Algiers have been arranged. Five of these each occupy a week, and one of them a fortnight. The great number of applications for tickets both from France and Spain compel us to imagine that in many cases the membership of the Society has been sought this year rather for the sake of the tempting excursions than for the love of science. April is one of the most lovely months in the year at Algiers: the mean temperature is 16·5° C., with a possible minimum of 8°, and a possible maximum of 30°. In May the mean temperature is 19·5° C., and there may be eight days of rain; while at Biskra the maximum may be as high as 40° C. (104° F.), and not more than one day of rain may be expected in May. A proclamation has been issued by the local committee asking the inhabitants to place rooms at the disposal of the visitors. Among those who will cross the Mediterranean will be Admiral Mouchez, MM. Quatrefages, Wurtz, Saporta, the naturalist, M. Cartaillac, the geologist, and many others, who will give interesting papers on a variety of subjects.

MR. ASHTON DILKE tried in vain on Tuesday to get the House of Commons seriously to consider the advisability of adopting the decimal system of coinage in this country. It is hopeless in the present state of public affairs to induce Parliament to attend to a matter of this kind. On the widely beneficial results of the adoption of the metric system in whole or in part we have often insisted. That there would be some inconvenience in making the transition, of course every one will admit; but as compared to the ultimate benefits from the adoption of the metric system, they are not worthy of consideration. Mr. Dilke does well not to let the matter drop out entirely of public notice.

THE Thore prize of the Académie des Sciences of Paris has been awarded to M. A. Vayssière, préparateur des cours de Zoologie à la Faculté des Sciences de Marseille, for an anatomical memoir of *Prosopistoma punctifrons*, Lat. Some of our readers interested in comparative anatomy may remember having seen the original drawings in London last summer, and will be glad to know that it will soon be forthcoming. M. Vayssière is a careful expert.

THE French Minister of Public Instruction intends to do a great service to science by publishing monthly a *résumé* of the scientific work being done over France, under the title of *Revue des Sciences*. The review will be under the direction of the venerable M. H. Milne-Edwards, and will consist exclusively of analyses and summaries, but of sufficient detail to give a fair idea of the nature of the work being done. It will embrace the work of individuals and of societies all over the country, and each number will contain about 100 pages.

M. DELESSE, a member of the Institute, vice-president of the Geographical Society of Paris, and author of a number of works and papers on geology, died in Paris at the age of sixty-three years.

THE death is announced on the 25th inst. of Sir Charles Reed, M.P., the much-respected chairman of the London School Board.

A METEOROLOGICAL observatory has been erected at Port-au-Prince, Haiti, under the care of the Rev. Father Wiek, on ground granted by the State. It is an octagon of two stories and a platform. Besides the indispensable instruments it has electric clocks (for communicating the time to clocks outside), telephones, microphones, phonographs, radiometers, &c.

THE inaugural meeting of a Society of Chemical Industry will be held in the rooms of the Chemical Society, Burlington House, Piccadilly, on April 4, at 4 p.m. This Society is not intended to represent any one particular branch of chemical industry. It is hoped that it will be representative of many manufactures—alkali-making, manure-making, the textile colour industries, the glass and pottery manufactures, tar distilling, soap-making, sugar-making, brewing, metallurgy, the manufacture of fine chemicals, and all other industries which show any connection with chemical science.

THE newly-issued part of the *Medical Reports* which are from time to time issued by order of the Inspector-General of Chinese Maritime Customs, contains an elaborate monograph by Dr. Duane B. Simmons on the subject of Beriberi, or the Kakké of Japan, which includes some interesting notes on the history and geographical distribution of the disease, and is illustrated by a sketch-map.

MR. BOWDLER SHARPE, F.L.S., delivered on Thursday last the concluding lecture of a series on the "Birds of the World," which he has been giving at Tonbridge School. Throughout the winter lectures have been given on various literary and scientific subjects by Prof. Henry Morley, Rev. A. Lucas, and others, and large and attentive audiences have shown great interest in all the series. The school already possesses a small museum, which is increasing under the auspices of the present head-master, the Rev. T. B. Rowe, who is evidently doing his best to encourage a taste for science and literature in the institution under his charge.

EVERY ornithologist should read a little pamphlet recently sent to us by the Dundee Naturalists' Society, entitled "The Grallatores and Natatores of the Estuary of the Tay; the great decrease in their numbers of late years; the causes; with suggestions for its mitigation. A paper read by Col. Drummond Hay." The author, whose long residence in the district alluded to renders his experiences doubly interesting, makes out a good case for his friends the birds in regard to their alleged destruction of fish and spawn, and no doubt some notice will be taken of his statements at the approaching Fisheries' Exhibition at Norwich. The principal cause in the decrease of the birds on the Tay he attributes chiefly to the increased number of gunners on the river, who disregard the close-season, while the wilful destruction of the sea-birds' eggs also plays sad havoc amongst their numbers. Drainage and cultivation of the land has also altered the conditions under which certain species nested, and has driven them further afield.

A Conference on the reform of the Educational Code is to meet in London in the third week in April, and to sit for two days, for the purpose of drawing up a series of recommendations, to be submitted in the form of a memorial to the Vice-President of the Committee of Council. The gentlemen invited to attend are persons conversant with the practical working of the public elementary school system, head-masters of secondary schools, persons experienced in education, and others interested. Invitations have been accepted by the chairmen of the Education and School Management Committees of the School Boards for London, Liverpool, Birmingham, Leeds, Sheffield, Bristol, Bradford, Leicester, and Nottingham; also by Dr. Abbott, Dr. Caldicott, Mr. Eve, Professors Max Müller, Carey Foster, Henrici, Gladstone, and Meiklejohn; Sir U. Kay-Shuttleworth, Sir. John Lubbock, the Rev. Mark Pattison, and numerous others.

MR. STEPHEN BRETTON, F.M.S., writes from Eastbourne to the *Times*, under date March 28, that he saw a meteor of great splendour that morning (1.15 a.m. Greenwich mean time), the finest he ever observed. Its size was apparently rather larger than Venus at her brightest, and for two or three seconds illuminated the heavens very brilliantly. Its colour was of an intense purple white, and moved somewhat slowly. He first noticed it a little south of Regulus, and going in direction of Castor. When immediately below Prœsepe it burst into about five or six fragments, each about the size of a star of the third or fourth magnitude, these assuming a deep fiery red. It then immediately disappeared. The night was especially clear; temperature in air about 30°; barometer about 29·850.

THE Committee of the "Frank Buckland Memorial Fund" have decided that the memorial shall take the form of a bust to be placed in the Fish Museum at South Kensington; the purchase of an annuity to be presented to Mrs. Buckland; and, if there be any surplus, it will be applied in some way to promote the welfare of the fishermen of this country. The honorary secretaries are Col. Bridges and Mr. T. Douglas Murray, to whom subscriptions may be sent at 34, Portland Place.

SMART shocks of earthquake occurred at Agram on March 21 at 3h. 40m. a.m., duration three seconds, and on March 24 at 6h. 45m. a.m., both accompanied by loud subterranean noises.

THERE was another earthquake shock at Casamicciola on Sunday morning at 6.45.

M. VAN MALDEREN, who was the electrical engineer of the Alliance, and constructed the so long unrivalled magneto-electric machine belonging to this Company, died at Brussels at the age of seventy a few days ago.

ALL the obstacles which have prevented the reconstruction of the Sorbonne being accomplished, have been removed by M. Jules Ferry, and the work will begin immediately. The same may be said of the isolation of the Public Library of Paris, all the required expropriations having been decreed.

THE date for admission of exhibits to the International Exhibition of Electricity at Paris has been prolonged to April 15.

THE Geologists' Association Easter Excursion will, be on Monday and Tuesday, April 18 and 19, to Salisbury, Stonehenge, and Vale of Wardour.

COLONEL PARIS, the head of the Paris fire brigade, has concluded his report on the destruction of the Printemps Establishment by proposing that large warehouses be compelled to light by electricity. The burning of the Nice Theatre, which was occasioned by a gas explosion, has given a new importance to that movement.

M. DE MERITENS has completed the construction of one of his magneto-electric engines intended for lighthouse illumination. An experimental trial took place on March 25 before MM. Becquerel, Cornu, Mascart, and other members of the Technical Commission of the International Exhibition. It was proved that with fifteen horse-power his machine illuminates at once more than thirty Jablochkoff lights, and that it could, at a moment's notice, be used in a regulator for marine purposes.

MR. THOMAS EDWARD, the Banff naturalist, has reprinted in a separate form some useful and interesting papers on the Protection of Wild Birds. The pamphlet is to be had at the *Banffshire Journal* Office.

THE additions to the Zoological Society's Gardens during the past week include an Egyptian Gazelle (*Gazella dorcas*) from Egypt, presented by the Earl of March, F.Z.S.; a Common Genet (*Genetta vulgaris*), South European, presented by the Rev. F. P. Voules; a Giant Toad (*Bufo agua*) from Brazil, presented by Mr. Carl Hagenbeck; a Long-snouted Snake (*Passerita mycterizans*) from India, presented by Mr. H. H. Black; an Amherst's Pheasant (*Thaumalea amherstiæ*) from Szechuen, China, a Black Swan (*Cygnus atratus*) from Australia, purchased; a Tiger (*Felis tigris*), a Bactrian Camel (*Camelus bactrianus*), a Sambur Deer (*Cervus aristotelis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

A NEW VARIABLE STAR.—On July 26, 27, and 29, 1783, D'Agelet observed a star which he twice estimated 6m., and on the last night 6·5m.; it is No. 5057-9 in Gould's reduced catalogue, the mean of the three observations giving for 1800, R.A. 19h. 23m. 47·57s. and Decl. + 17° 19′ 42″·8. The only subsequent observation we have yet found of this star is in the *Durchmusterung*, where it is rated as low as 9·4m.; there is consequently a high probability that it will prove to be a remarkable variable. The position brought up to the beginning of 1880 will be R.A. 19h. 27m. 22·1s., Decl. + 17° 29′ 28″. D'Agelet's original observations will be found at pp. 542, 544, and 546 of the *Histoire Céleste* of Lalande.

MINIMA OF ALGOL, ETC., IN 1880.—Prof. Julius Schmidt has published his observations, or rather the results of his observations, of Algol and other variable stars, made at Athens during the past year. On comparing his epochs of minima with the formula in Prof. Schönfeld's last catalogue, it will be found that according to the most completely determined minima the calculation is too late by nearly half an hour. But the differences between calculation and observation are very irregular, so that if we take a mean of the whole, the true minimum would appear to be earlier than that computed by only nineteen minutes. The minima between August 28 and December 21 are here compared.

According to the observations of the same indefatigable astronomer *Mira Ceti* was at a maximum between July 20 and 25, but in 1880 it only attained about 4·2 m. A maximum of R Leporis occurred about November 9; the determination is not very certain. The intervals between maximum and minimum, and *vice versâ* of α Herculis were as irregular as usual.

THE RED SPOT UPON JUPITER'S DISK.—Dr. Jedrzejewicz has published some inferences from observations for ascertaining the time of rotation of the eastern extremity of the large red spot upon the disk of Jupiter, made at his private observatory at Plonsk during the winter of 1880-81. The instrument employed is a refractor six-inches aperture, with powers 225 to 300. In December he measured the length of the spot 9″·8, and considers that his own observations compared with those of Prof. Schmidt at Athens, indicate that the length of the spot remained unchanged during the winter. On this assumption he finds for the time of rotation 9h. 55m. 34·414s. ± 0·13s, by 174 rotations between November 25, 1880, and February 5, 1881. Prof. Schmidt from 1021 rotations between July 23, 1879, and September 17, 1880, obtained the value 9h. 55m. 34·422s. ± 0·05s. for the middle of the spot. In 1862, by observations upon a spot which he says was much darker and a more favourable object for the purpose than the spots observed by Airy and Mädler in 1834-35, and which was not much larger than the shadow of the third satellite he had found for the time of rotation 9h. 55m. 25·684s. agreeing with the previously-determined values. While the period from observations of the red spot is 9s. greater, Prof. Schmidt remarks that it agrees very nearly with that already obtained by Mr. Pratt.

THE MINOR PLANETS.—It appears that the object detected by Herr Palisa at the new Observatory of Vienna on the 23rd of last month, and which was announced as No. 220 of the small-planet group, may prove to be No. 139 *Juewa*, which had not been observed since 1874. It was discovered by the late Prof. Watson at Pekin on October 10 in that year, while he was engaged upon one of the United States expeditions for the observation of the transit of Venus, and as was reported at the time, without the aid of a chart of telescopic stars, but from his memory of their configuration about the particular spot occupied by the planet. It was observed on November 8 by Rümker at Hamburg, but the length of observation was not sufficient to determine the mean motion with any degree of accuracy: hence although the elements had been several times brought up to more recent dates by Watson, the planet had not been recovered up to last month.

By the last Berlin circular it would seem that *Ismene* will fall little short of *Hilda* in the length of its revolution, and these two minors will thus stand out as exceptional members of the group. By the latest elements the period of *Hilda* is 2860 days or 7·832 years, and that of *Ismene* 2854 days or 7·814 years.

Calculation has assigned the shortest period to No. 149 *Medusa*, but this awaits confirmation, perhaps in the next summer, when the planet should again come into opposition according to the imperfect elements at present available.

PHYSICAL NOTES

M. PLANTAMOUR continues to study with his sensitive levels the phenomena of periodic rise and fall of the ground which he has observed in Switzerland. He believes he has established a connection between these periods and those of the changes of temperature of the earth's surface, there being an annual change of level in an east-west direction corresponding with the mean temperatures of the surface during the year.

M. ROSENSTIEHL concludes from his researches on the sensations of colour recently noticed that the three fundamental colour sensations of the Young-Helmholtz-Maxwell theory correspond to the following tints of the pure spectrum. *Orange-red*, three-fourths of the distance from C to D amongst the Fraunhofer lines, a *yellow-green* three-quarters of the distance from D to E, and a *blue* situated at one-third from F towards G. The principle upon which this selection is made is that the selected tint fulfils the following conditions: (*a*) it is equidistant between two tints which are complementary to one another; (*b*) it produces with either of the other selected tints another colour having a minimum of white admixed with it. Thus the yellow-green chosen is midway between that yellow and that blue which produce the best white with one another, and it gives with the selected orange-red a yellow more intense than any known yellow pigment under equal illumination, and with the selected blue gives a green more intense than the richest green pigment.

M. HENRI BECQUEREL observes that the specific magnetism of ozone exceeds that of oxygen, and is much greater than could be accounted for by the difference in density of these two allotropic forms of the gas.

IN view of recent terrible colliery explosions in Belgium, M. Cornet has called attention (in the Belgian Academy) to a possible interference of winds, blowing in an inclined direction, with the proper ventilation of mines. Most of the "fiery" Belgian mines have two shafts, one for raising the coal and for descent of air, which, passing along the galleries, is drawn up the other shaft by a ventilating engine. The orifice of the latter shaft is generally (unlike that of the other) unsheltered by buildings; it debouches directly in the air a little above the ground. Obviously, then, a strong wind, blowing with downward inclination towards this orifice, might seriously affect the ventilating action. It is noted that one explosion in Hainaut on November 19, 1880, followed a night of very high wind, which M. Cornet shows to have been capable of depressing ventilation considerably. Mines with large sections are more dangerous than others in atmospheric perturbations. The true remedy, however (in the author's opinion), is not increasing the resistance to the air-currents, but sheltering the orifices of the ventilating shafts against descending winds.

IN a recent paper on the optical structure of ice (to the Freiburg Society of Naturalists) Prof. Klocke finds that while in the ice individuals the plane of the secondary axes is fixed by the position of the principal axis, they are subject to no law as to direction in that plane.

THE phenomenon of *verglas* occurred at Urbino in Italy twice in January; and from his observations of it Prof. Serpieri con-

cludes (*Real. Ist. Lomb. Rend.*) that surfusion of the rain-drops is not indispensable to its production. Surfusion indeed accelerates it, as do also violence of wind and intense cold; but a rain with temperature not so low as zero falling into an air-current in rapid motion and below zero gives the phenomenon. It is pointed out, however, that the mist which usually accompanies verglas being driven against objects by the wind, and its particles being in a state of surfusion (the temperature being below zero), probably contributes to the general result, helping to make the ice-layer regular and uniform. If the verglas be such that the drop freezes wholly at once, the latter has probably contained many small crystals of ice.

M. MERCADIER sums up his researches on Radiophony by saying that he believes that the phenomena are due to a vibratory movement set up by the alternate heating and cooling, due to the intermittent beams of heat-rays, of the gaseous layer adjacent to the solid surface at which the radiations are absorbed; being an anterior layer in the case of solid bodies, a posterior layer in the case of transparent bodies.

M. JANSSEN has succeeded in photographing the *lumière cendrée*, or "earth-shine" on the moon when three days old: in the photograph the "continents" were to be distinguished clearly from the "seas." This disposes of the view sometimes advanced, and held, we believe, by some most eminent astronomers, that the "new moon in the arms of the old" was an optical illusion.

PROF. D.-W. BEETZ, of the Technical High School of Munich, wishes us to say that in the note (vol. xxiii. p. 442) on the modulus of elasticity of rods of carbon, he, and not Herr Holtz, should have been mentioned as the author of the paper on the subject in *Wied. Ann.*

GEOGRAPHICAL NOTES

AT the meeting of the Geographical Society on Monday Mr. J. B. Minchin, who has spent some seven years in the country, read an excellent paper on Eastern Bolivia, which also included some observations on the Gran Chaco. After some preliminary remarks Mr. Minchin dwelt at length on the water-system of the country, and, speaking first of the lakes, he mentioned that between the Rivers Pilcomayo and Paraguay, in the unexplored Chaco, the Indians report the existence of a lake which no white man has ever yet seen, but which is perhaps near 22° S. lat. The rivers belong to Amazon and Plate systems, and with the exception of the Paraguay and the Itenez, they mostly have their sources among the highest summits of the Andes. The Parapite, Mr. Minchin added, is the most southerly affluent of the Amazons, which in some maps has been made to flow across the Chaco into the Paraguay. The Pilcomayo also does not, as has been thought, receive any tributaries on its course through the Chaco, so far as can be learned from the Indians. Mr. Minchin afterwards alluded to his expedition over the Matto Grossi Mountains, which he succeeded in crossing for the first time. The latter part of the paper was largely devoted to the animal and vegetable productions of Eastern Bolivia and to the commercial condition of the country. The discussion which followed turned chiefly on the route of the future into Bolivia, whether it would be most advantageous to follow the Paraguay route or develop a new one by the Madera.

MR. E. G. RAVENSTEIN has nearly completed for the Council of the Geographical Society the large map of Eastern Equatorial Africa, on which he has been engaged for nearly three years under the direction of their Scientific Purposes Committee. The original drawings will be reduced before they are engraved, and the map when published will be in twenty-four sheets, and on a scale of 1 : 1,000,000. It will take in the lake region, the Upper Congo, and the Upper Nile, and on the east coast will extend from Somali Land to a little south of the Zambesi, the precise limits of the map being from 10° N. to 20° S. lat., and from 25° to 52° E. long. A very complete bibliography of authorities, compiled *pari passu* with the map, will be published afterwards.

MR. BROUMTON, an agent of the China Inland Mission at Kweiyang-fu, in the province of Kweichow, has lately sent home an account of a visit which he had paid by invitation to the Miao-tsze tribes a short distance off. He had been told by one of them, from whom he had been learning something of the language, that in the third moon of the year his people had large gatherings in the hills, and was asked to be present at these festivities. He accordingly went and had an excellent opportunity for observing the manners and customs of this section of this comparatively unknown people. He describes their dress, the character of the festivities witnessed, the singular musical instruments used, &c. The particular tribes visited by Mr. Broumton are known as the Black (from the colour of their clothes) and the Ka-teo tribes, and live near Hwangping-chow.

MR. CARL BOCK is leaving for Siam next week, where he intends to make an excursion into the interior. His book, "The Headhunters of Borneo," will be published shortly by Messrs. Sampson Low and Co.

WE hear that Mr. Edward Whymper, who has already given an account of some phases of his South American journey to the Alpine Club and the Society of Arts, will read a paper on the Andes of Ecuador before the Geographical Society on May 9.

PRIZES OF THE PARIS ACADEMY OF SCIENCES

AT the public *séance* of the Academy on March 14 the annual distribution of prizes took place. While many of these prizes are offered for particular subjects, others are devoted to rewarding the most important advances made during the year in special departments of science.

The Grand Prize of the Mathematical Sciences was awarded to M. Halphen for work on the theory of linear differential equations.

In astronomy Mr. Stone receives the Lalande prize for his stellar researches, following those of Abbé de Lacaille, at the Cape of Good Hope; and the Valz prize goes to M. Tempel for his observations on comets. M. Vinot's labours in starting and editing *Le Ciel* are recognised by the award of the Tremont prize.

The Montyon prize of the mechanical arts is given to M. Cornut for his study of the faults of iron plates; the Poncelet prize to M. Leauté for various works; while a recompense of 1500 francs on the Bordin foundation is given to M. Lan for improved modes of combustion, diminishing the trouble and harm from smoke, &c. (in steel heating). The extraordinary prize of 6000 francs (for improving the efficacy of naval forces), and the Plumey prize, are not awarded.

In physics we find a recompense of 3000 francs given to M. Ader, on the Vaillant foundation, for improvements in phonetic telegraphy. The grand prize for researches on elasticity of crystalline bodies is not awarded.

The Jecker prize goes to M. Demarçay for important work in organic chemistry; the Gegner prize to M. Jacquelain for skill in preparing a large number of substances in a pure state, &c.

Two prizes on the Bordin foundation are awarded in geology, one to M. Gosselet for a geological sketch of the North of France, the other to MM. Falsan and Chantre for their geological monography of ancient glaciers and erratic deposits in the middle of the Rhone Valley. Recompenses on the Gay foundation are awarded to MM. Delage and Chevremont for observations on movements of the coast-line in France.

In medicine and surgery three Montyon prizes are awarded: one to Dr. Charcot for his work on localisation of disorders of the brain; another to Dr. Sappery for researches on the lymphatic apparatus of fishes; the third to Dr. Jullien for important medical researches. On the Bréant foundation M. Colin is awarded 5000 francs for physiological researches. Dr. Segond receives the Godard prize for an important work in surgery; Dr. Quinand the Barbier prize for researches on the quantity of oxygen in human blood in health and in disease. The Dusgate prize (having regard to prevention of premature burial) is not given, but MM. Onimus, Peyrand, and Le Bon are recompensed for their researches. The Boudet prize is awarded to Prof. Lister.

In experimental physiology the Montyon prize is given to M. Bonnier for researches on the nectaries and colours of plants.

The Fons-Melicocq prize for botanical research in the north of France is gained by M. de Vicq; and M. de la Chapelle receives 1000 francs on the Desmazières foundation for studies on French cryptogams.

In anatomy and zoology the Grand Prize with reference to distribution of marine animals on the French coast is withheld.

M. Grandidier gains the Savigny prize for researches on the fauna of Zanzibar and Madagascar; while the Thorel prize is awarded both to M. Vayssières and M. Joly, for observations proving a small animal found in streams to be the larva of an insect of the family of Ephemerans.

The Montyon prize for statistics goes to Dr. Ricoux for his "Figured Demography of Algeria."

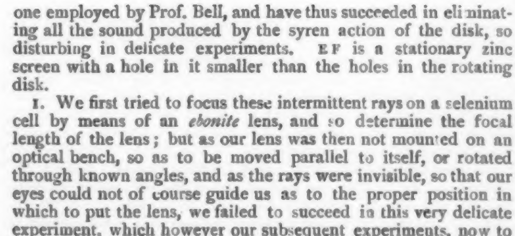
We further note that M. Birckel receives 1500 francs on the Montyon foundation, for an improvement in the Davy lamp, and that M. Dupuis receives the Delaland-Guerineau prize for his explorations in Tonkin.

The published list of subjects for prize competition in 1881, 1882, 1883, and 1885 comprises the following subjects (briefly stated) among others:—Motor for tramways; physiology of champignons; influence of environment on plant-organs; structure and development of cork; internal organisation of European edriophthalmate crustaceans; cure of Asiatic cholera; genito-urinary organs; revision of the theory of Jupiter's satellites; elasticity of crystalline bodies; origin of atmospheric electricity, and causes of electric phenomena in thunderstorms; inoculation as a prophylactic for domestic animals; coloured parts of the tegumentary system of animals, and fecundating matter of animated beings; marine, lacustrine, and terrestrial deposits on the French coast since the Roman epoch; botany of the North of France; diagnostic signs of death and prevention of premature burial.

MEASURING THE INDEX OF REFRACTION OF EBONITE [1]

PROF. BELL found that when an intermittent beam of light fell on a sensitive selenium cell the sound produced in a telephone (which with a battery was attached to the selenium) was not entirely destroyed by interposing a thin sheet of ebonite in the path of the intermittent rays of light, or, in other words, that ebonite was slightly transparent for invisible rays that affected selenium. It occurred to us some months ago that if such invisible rays were at all of the nature of light, they probably suffered retardation in passing through the ebonite, or that refraction would take place if the sheet of ebonite were replaced by an ebonite prism or lens, a result we have been able experimentally to confirm, and at the same time to measure the index of refraction.

A B is a glass lens concentrating a parallel beam coming from a lime-light on to one hole H in a rapidly revolving brass disk C D. This disk we have constructed many times as thick as the



one employed by Prof. Bell, and have thus succeeded in eliminating all the sound produced by the syren action of the disk, so disturbing in delicate experiments. E F is a stationary zinc screen with a hole in it smaller than the holes in the rotating disk.

1. We first tried to focus these intermittent rays on a selenium cell by means of an *ebonite* lens, and so determine the focal length of the lens; but as our lens was then not mounted on an optical bench, so as to be moved parallel to itself, or rotated through known angles, and as the rays were invisible, so that our eyes could not of course guide us as to the proper position in which to put the lens, we failed to succeed in this very delicate experiment, which however our subsequent experiments, now to be described, show must ultimately succeed with the lens properly mounted.

2. A small portion of the intermittent light which passed through the hole H in the rotatory disk was allowed to fall on an ebonite prism K L, by passing through a slit in a zinc screen G J, the slit being arranged parallel to the edge of the ebonite prism. The prism employed had an angle of 27°·5. M N is another zinc screen with a slit in it also parallel to the edge of the prism, and placed in front of a sensitive selenium cell S (the cell described by us in the account of our experiments on "Seeing by Electricity"). This screen M N was moved parallel to itself, while an experimenter listened with a telephone to each ear, and who was placed in another room, so as not to be influenced by seeing what changes were being made in the position of the screen or in the position of the ebonite prism. The telephones had each a resistance of 74 ohms, and the battery an electromotive force of about 40 volts. No direct light falling on the selenium, the listener at the telephones heard nothing for the majority of positions of the screen M N, but in one position represented in the figure a faint distant sound was distinctly heard, which was entirely cut off by interposing the hand in front of the selenium, or by moving away the prism.

The invisible rays that affect selenium after passing through ebonite are consequently refracted, and some preliminary experiments, when the ebonite prism was arranged for minimum deviation, gave 1·7 as a first rough approximation for the index of refraction of these rays by ebonite. It is interesting to notice that the square, 2·89, of this index of refraction is between the highest and lowest limits obtained by different experimenters for the specific inductive capacity of ebonite, so far agreeing with Maxwell's electromagnetic theory of light.

We are now having prisms of ebonite and of other opaque substances of different angles mounted on a goniometer stand, to enable us to measure the indices of refraction accurately.

[1] Note communicated to the Royal Society by Professors Ayrton and Perry.

MOLECULAR ELECTROMAGNETIC INDUCTION [1]

THE induction-currents balance which I had the honour of bringing before the notice of the Royal Society (*Proc.* Roy. Soc. vol. xxix. p. 56) showed how extremely sensitive it was to the slightest molecular change in the composition of any metal or alloy, and it gave strong evidence of a peculiarity in iron and steel which its magnetic properties alone failed to account for. We could with all non-magnetic metals easily obtain a perfect balance of force by an equivalent piece of the same metal, but in the case of iron, steel, and nickel it was with extreme difficulty that I could obtain a near approach to a perfect zero. Two pieces of iron cut off the same bar or wire, possessing the same magnetic moment, never gave identical results; the difficulty consisted, that notwithstanding each bar or wire could be easily made to produce the same inductive reaction, the time during which this reaction took place varied in each bar; and although I could easily change its balancing power as regards inductive force by a change in the mass of the metal, by heat or magnetism, the zero obtained was never equal to that obtained from copper or silver.

This led me to suppose the existence of a peculiarity in magnetic metals which could not be accounted for except upon the hypothesis that there was a cause, then unknown, to produce the invariable effect.

Finding that it would be impossible to arrive at the true cause without some new method of investigation, which should allow me to observe the effects from an electrical circuit, whose active portion should be the iron wire itself, I constructed an apparatus or electro-magnetic induction balance, consisting of a single coil reacting upon an iron wire in its axis, and perpendicular to the coil itself; by this means the iron or other wire itself became a primary or secondary, according as the current passed through the coil or wire. Now with this apparatus we could induce secondary currents upon the wire or coil, if the coil was at any angle, except when the wire was absolutely perpendicular; in this state we could only obtain a current from some disturbing cause, and the current so obtained was no longer secondary but tertiary.

The whole apparatus however is more complicated than the general idea given above, as it was requisite not only to produce effects, but to be able to appreciate the direction of the electrical current obtained, and have comparative measures of their value. In order to fully understand the mode of experiments, as well as the results obtained, I will first describe the apparatus employed.

The electro-magnetic induction balance consists—(1) of an

[1] Paper read at the Royal Society, March 17, by Prof. D. E. Hughes, F.R.S.

instrument for producing the new induction-current; (2) sonometer or balancing coils; (3) rheotome and battery; (4) telephone.

The essential portion of this new balance is that wherein a coil is so arranged that a wire of iron or copper can pass freely through and forming its axis, the iron or copper wire rests upon two supports 20 centims. apart; at one of these the wire is firmly clamped by two binding screws; the opposite end of the wire turns freely on its support, the wire being 22 centims. long, having 2 centims. projection beyond its support, in order to fasten upon it a key or arm which shall serve as a pointer upon a circle giving the degrees of torsion which the wire receives from turning this pointer. A binding screw allows us to fasten the pointer at any degree, and thus preserve the required stress the time required.

The exterior diameter of the coil is $5\frac{1}{4}$ centims., having an interior vacant circular space of $3\frac{1}{2}$ centims., its width is 2 centims.; upon this is wound 200 metres of No. 32 silk-covered copper wire. This coil is fastened to a small board so arranged that it can be turned through any desired angle in relation to the iron wire which passes through its centre, and it can also be moved to any portion of the 20 centims. of wire, in order that different portions of the same wire may be tested for a similar stress.

The whole of this instrument, as far as possible, should be constructed of wood, in order to avoid all disturbing inductive influences of the coil.

The iron wire at its fixed end is joined or makes contact with a copper wire, which returns to the front part of the dial under its board and parallel to its coil, thus forming a loop, the free end of the iron wire is joined to one pole of the battery, the copper wire under the board is joined to the rheotome and thence to the battery.

The coil is joined to the telephone; but, as in every instance we can either pass the battery through the wire, listening to its inductive effects upon the wire, or the reverse of this, I prefer, generally, in order to have no voltaic current passing through the wire, to join the iron wire and its loop direct to the telephone, passing the voltaic current through the coil.

In order to balance, measure, and know the direction of the new induction currents by means of a switching key, the sonometer (*Proc.* Roy. Soc., vol. xxix. p. 65) I described to the Royal Society is brought into the circuit. The two exterior coils of the sonometer are then in the circuit of the battery, and of the coil upon the board containing the iron wire or stress bridge. The interior or movable coil of the sonometer is then in the circuit of the iron wire and telephone. Instead of the sonometer constructed as described in my paper to the Royal Society, I prefer to use one formed upon a principle I described in *Comptes rendus*, December 30, 1878. This consists of two coils only, one of which is smaller and turns freely in the centre of the outside coil. The exterior coil being stationary, the centre coil turns upon an axle by means of a long (20 centims.) arm or pointer, the point of which moves over a graduated arc or circle. Whenever the axis of the interior coil is perpendicular to the exterior coil no induction takes place, and we have a perfect zero; by turning the interior coil through any degree we have a current proportional to this angle, and in the direction in which it is turned. As this instrument obeys all the well-known laws for galvanometers, the readings and evaluations are easy and rapid.

If the coil upon the stress bridge is perpendicular to the iron wire, and if the sonometer coil is at zero, no currents or sounds in the telephone will be perceived, but the slightest current in the iron wire produced by torsion will at once be heard; and by moving the sonometer coil in a direction corresponding to the current, a new zero will be obtained, which will not only balance the force of the new current, but indicate its value. A perfect zero however will not be obtained with the powerful currents obtained by the torsion of 2 millims. diameter iron wire; we then require special arrangements of the sonometer, which are too complicated to describe here.

The rheotome is a clockwork having a rapid revolving wheel which gives interruptions of currents in fixed cadences in order to have equal intervals of sound and silence. I employ four bichromate cells or eight Daniell's elements, and they are joined through this rheotome to the coil on the stress bridge, as I have already described.

The magnetic properties of iron, steel, nickel, and cobalt have been so searchingly investigated by ancient as well as by modern scientific authors, that there seems little left to be known as regards its molar magnetism. I use the word molar here simply to distinguish or separate the idea of a magnetic bar of iron or steel magnetised longitudinally or transversely from the polarised molecules which are supposed to produce its external magnetic effects.

Molar magnetism, whilst having the power of inducing an electric current in an adjacent wire, provided that either has motion or a change in its magnetic force, as shown by Faraday in 1832, has no power of inducing an electric current upon itself or its own molar constituent, either by motion or change of its magnetic moment. Molecular magnetism (the results of which I believe I have been the first to obtain) has no, or a very feeble, power of inducing either magnetism or an electric current in an adjacent wire, but it possesses the remarkable power of strongly reacting upon its own molar wire, inducing (comparatively with its length) powerful electric currents, in a circuit of which this forms a part.

We may have also both cases existing in the same wire; this occurs when the wire is under the influence of stress, either external or internal; it would have been most difficult to separate these two, as it was in my experiments with the induction balance without the aid of my new method.

Ampère's theory supposes a molecular magnetism or polarity, and that molar magnetism would be produced when the molecular magnetism became symmetrical; and his theory I believe is fully capable of explaining the effects I have obtained, if we admit that we can rotate the paths of the polarised molecules by an elastic torsion.

Matteucci made use of an inducing and secondary coil in the year 1847 (*Compt. rend.* t. xxiv. p. 301, 1847), by means of which he observed that mechanical strains increased or decreased the magnetism of a bar inside this coil.

Wertheim published in the *Comptes rendus*, 1852 (*Compt. rend.* t. xxv. p. 702, 1852), some results similar to Matteucci; but in the *Annales de Chimie et de Physique*, 1857 (*Ann. de Chim. et de Phys.* (3) t. l. p. 385, 3857), he published a long series of most remarkable experiments, in which he clearly proves the influence of torsion upon the increment or decrement of a magnetical wire.

Vilari showed (*Poggendorff's Annalen*, 1868) increase or diminution of magnetism by longitudinal pull according as the magnetising force is less or greater than a certain critical value.

Wiedermann (Wiedermann's "Galvanismus," p. 447), in his remarkable work, "Galvanismus," says that an iron wire through which an electric current is flowing becomes magnetised by twisting the wire. This I have repeated, but found the effects very weak, no doubt due to the weak battery I use, viz. four quart bichromate cells.

Sir W. Thomson shows clearly in his remarkable contribution to the *Phil. Trans.* Roy. Soc., entitled "Effects of Stress on the Magnetisation of Iron, Nickel, and Cobalt" (*Phil. Trans.* May 6, 1878), the critical value of the magnetisation of these metals under varying stress, and also explains the longitudinal magnetism produced by Wiedermann as due to the outside molar twist of the wire, making the current pass as in a spiral round a fixed centre. Sir William Thomson also shows clearly the effects of longitudinal as well as transversal strains, both as regards its molar magnetism and its electric conductivity.

My own researches convince me that we have in molecular magnetism a distinct and separate form of magnetism from that when we develop, or render evident, longitudinal or transversal magnetism, which I have before defined as molar.

Molecular magnetism is developed by any slight strain or twist other than longitudinal, and it is only developed by an elastic twist; for however much we may twist a wire, provided that its fibres are not separated, we shall only have the result due to the reaction of its remaining elasticity.

If we place an iron wire, say 20 centims. long, 1 millim. diameter, in the axis of the coil of the electro-magnetic balance, and if this wire is joined, as described, to the telephone, we find that on passing an electric current through the inducing coil no current is perceptible upon the iron wire; but if we give a very slight twist to this wire at its free end—one-eighteenth of a turn, or 20°—we at once hear, clear and comparatively loud, the currents passing the coil; and although we only gave a slight elastic twist of 20° of a whole turn, and this spread over 20 centims. in length, making an extremely slight molar spiral; yet the effects are more powerful than if, using a wire free from stress, we

turned the whole coil 40°. The current obtained when we turn the coil, as just mentioned, is secondary, and with the coil at any angle any current produced by its action, either on a copper, silver, iron, or steel wire; in fact it is simply Faraday's discovery, but the current from an elastic twist is no longer secondary under the same conditions, but tertiary, as I shall demonstrate later on. The current passing through the coil cannot induce a current upon a wire perpendicular to itself, but the molecules of the outside of the wire, being under a greater elastic stress than the wire itself, they are no longer perpendicular to the centre of the wire, and consequently they react upon this wire as separate magnets would upon an adjacent wire. It might here be readily supposed that a wire having several twists, so a fixed molar twist of a given amount would produce similar effects. It however does not, for in most cases the current obtained from the molar twists are in a contrary direction to that of the elastic torsion. Thus, if I place an iron wire under a right-handed elastic twist of 20° I find a positive current of 50° sonometer; but if I continue this twist so that the index makes one or several entire revolutions, thus giving a permanent molar twist of several turns, I find upon leaving the index free from any elastic torsion, that I have a permanent current of 10°, but it is no longer positive but negative, requiring that we should give an elastic torsion in the previous direction, in order to produce a positive current. Here a permanent elastic torsion of the molecules is set up in the contrary direction to its molar twist, and we have a negative current, overpowering any positive current which should have been due to the twisted wire.

The following table shows the influence of a permanent twist, and that the current obtained when the wire was freed from its elastic torsion was in opposition to that which should have been produced by the permanent twist. Thus a well-softened iron wire 1 millim. in diameter, giving 60° positive current for a right-handed elastic torsion of 20°, gave after 1°·80 permanent torsion a negative current of 10°.

1 complete permanent torsion	(right-handed)	negative	...	10
2	,,	,,	...	15
3	,,	,,	...	15
4	,,	,,	...	16
5	,,	,,	...	12
6	,,	,,	...	10
7	,,	,,	...	5
8	,,	,,	...	4
9	,,	,,	...	3
10	,,	,,	...	3

At this point the fibres of a soft wire commence to separate, and we have no longer a complete single wire, but a helix of separate wires upon a central structure.

If now, instead of passing the current through the coil, I pass it through the wire, and place the telephone upon the coil circuit, I find that I obtain equally as strong tertiary currents upon the coil as in the previous case, although in the first case there was produced longitudinal electro-magnetism in the perpendicular wire by the action of the coil, but in the latter case none or the most feeble electro-magnetism was produced, yet in these two distinct cases we have a powerful current produced not only upon its own wire, but upon the coil, thus proving that the effects are equally produced both on the wire and coil.

If we desire, however, in these reversible effects to produce in both cases the same electromotive force, we must remember that the tertiary current when reacting upon its own short wire produces a current of great quantity, the coil one of comparative higher intensity. We can, however, easily convert the great quantity of the wire into one of higher tension by passing it through the primary of a small induction coil whose resistance is not greater than one ohm. We can join our telephone, which may be then one of a high resistance, to the secondary of this induction coil, and by this means, and without changing the resistance of the telephone, receive the same amount of force, either from the iron wire or the coil.

Finding that iron, steel, and all magnetic metals produce a current by a slight twist, if now we replace this wire by one of copper or non-magnetic metals we have no current whatever by an elastic twist, and no effects, except when the wire itself is twisted spirally in helix; and whatever current we may obtain from copper, &c., no matter if from its being in spiral or from not being perpendicular to the axis of the coils, the currents obtained will be invariably secondary and not tertiary. If we replace the copper by an iron wire, and give it a certain fixed torsion, not passing its limit of elasticity, we find that no increase or decrease takes place by long action or time of being under strain. Thus a wire which gave a sonometric force of 50° at the first observation remained perfectly constant for several days until it was again brought to zero by taking off the strain it had received. Thus we may consider that as long as the wire preserves its elasticity, exactly in the same ratio will it preserve the molecular character of its magnetism.

It is not necessary to use a wire to produce these effects; still more powerful currents are generated in bars, ribbons, or sheets of iron; thus no matter what external form it may possess, it still produces all the effects I have described.

It requires a great many permanent twists in a wire to be able to see any effect from these twists, but if we give to a wire, 1 millim. diameter, forty whole turns (or until its fibres become separated) we find some new effects; we find a small current of 10° in the same direction as its molar twist, and on giving a slight twist (20°) the sonometric value of the sound obtained is 80° instead of 50°, the real value of a similar untwisted wire; but its explanation will be found by twisting the wire in a contrary direction to its molar twist. We can now approach the zero but never produce a current in the contrary direction, owing to the fact that by the spiral direction, due to the fibrous molar turns, the neutral position of its molecule is no longer parallel with its wire, but parallel with its molar twist, consequently an elastic strain in the latter case can only bring the molecules parallel with its wire, producing no current, and in the first case the angle at which the reaction takes place is greater than before, consequently the increased value of its current.

The measurements of electric force mentioned in this paper are all sonometric on an arbitrary scale. Their absolute value has not yet been obtained, as we do not, at our present stage, require any except comparative measures.[1] Thus, if each wire is of 1 millim. diameter and 20 centims. long, all render the same stress in the axis of its coil. I find that the following are the sonometric degrees of value:—

Soft iron	60°	Tertiary current.
Hard drawn iron...	50	,, ,,
Soft steel	45	,, ,,
Hard tempered steel	10	,, ,,
Copper, silver, &c.	–0	,, ,,
Copper helix, 1 centim. diameter, 20 turns in 20 centims.	45	Secondary currents.
Iron, spiral, ditto	45	,, ,,
Steel	45	,, ,,

The tertiary current increases with the diameter of the wire, the ratio of which has not yet been determined; thus an ordinary hard iron wire of 1 millim. diameter giving 50°, one of 2 millims. diameter gave 100°; and the maximum of force obtained by any degree of torsion is at or near its limit of elasticity, as if in the same time we also pass this point, producing a permanent twist, the current decreases, as I have already shown in the case of a permanent twist. Thus, the critical point of 1 millim. hard iron wire was 20° of torsion, but in hard steel it was 45°.

Longitudinal strains do not produce any current whatever, but a very slight twist to a wire, under a longitudinal strain, produces its maximum effects: thus, 20° of torsion being the critical point of iron wire, the same wire, under longitudinal strain, required but from 10° to 15°. It is very difficult however to produce a perfect longitudinal strain alone. I have therefore only been able to try the effect of longitudinal strain on fine wires, not larger than 1 millim in diameter, but as in all cases no effect whatever was produced by longitudinal strain alone, I believe none will be found if absolutely free from torsion. The molecules in a longitudinal strain are equally under an elastic strain as in torsion, but the path of their motion is now parallel with its wire, or the zero of electric inductive effect, but the compound strain composed of longitudinal and transverse, react upon each other, producing the increased effect due to the compound strain.

The sonometer is not only useful for showing the direction of the current and measuring it by the zero method, but it also shows at once if the current measured is secondary or tertiary. If the current is secondary its period of action coincides with that of the sonometer, and a perfect balance, or zero of sound, is at once obtained, and its value in sonometric degrees given, but if the current is tertiary, no zero is possible, and if the value of the tertiary is 60°, we find 60° the nearest approach to zero

[1] 50° sonometer has the same electromotive force as 0°·10 of a Daniell battery.

possible. But by the aid of separate induction coils to convert the secondary into a tertiary, a perfect zero can be obtained if the time of action and its force correspond to that which we wish to measure.

If I place a copper wire in the balance and turn the coils at an angle of 45°, I obtain a current which can be perfectly balanced by the sonometer at 50°, proving, as already said, that it is secondary. If I now replace the copper by an iron wire, the coil remaining at 45°, I have again exactly the same value for the iron as copper, viz., 50°, and in both cases secondary. Now, it is evident that in the case of the iron wire there was produced at each passage of the current a strong electro-magnet, but this longitudinal magnetism did not either change the character of the current or its value in force.

A most beautiful demonstration of the fact that longitudinal magnetism produces no current, but that molecular magnetism can act equally as well, no matter the direction of the longitudinal magnetism, consists in forming an iron wire in a loop, or taking two parallel but separate wires, joined electrically at their fixed ends, the free ends being each connected with the circuit, so that the current generated must pass up one wire and down the adjacent one. On testing this loop, and if there are no internal strains, complete silence or absence of current will be found. Now, giving a slight torsion to one of these wires in a given direction, we find, say 50° positive; twisting the parallel wire in a similar direction produces a perfect zero, thus, the current of the second must have balanced the positive of the first. If, instead of twisting it in similar directions, we twist it in the contrary direction, the sounds are increased in value from 50° positive to 100° positive, showing, in this latter case, not only a twofold increase of force, but that the currents in the iron wires travelled up one wire and down the other, notwithstanding that both were strongly magnetic by the influence of the coil in one direction, and this experiment also proves that its molar magnetism had no effect, as the currents are equally strong in both directions, and both wires can double or efface the currents produced in each. If instead of two wires we take four, we can produce a zero, or a current of 200°, and with twenty wires we have a force of 1000°, or an electromotive force of two volts. We have here a means of multiplying the effects by giving an elastic torsion to each separate wire, and joining them electrically in tension. If loops are formed of one iron and one copper wire, we can obtain both currents from the iron wire, positive and negative, but none from the copper, its *rôle* is simply that of a conductor upon which torsion has no effect.

I have already mentioned that internal strains will give out tertiary currents without any external elastic strain being put on. In the case of iron wire these disappear by a few twists in both directions, but in flat bars or forged iron they are more permanent; evidently portions of these bars have an elastic strain, whilst other portions are free, for I find a difference at every inch tested: the instrument however is so admirably sensitive and able to point out not only the strain but its direction, that I have no doubt its application to large forged pieces, such as shafts or cannon, would bring out most interesting results, besides its practical utility; great care is therefore necessary in these experiments that we have a wire free from internal strains, or that we know their value.

Magnetising the iron wire by a large steel permanent magnet has no effect whatever. A hard steel wire thus placed becomes strongly magnetic, but no current is generated, nor has it any influence upon the results obtained from molecular movement, as in elastic torsion. A flat wide iron or steel bar shows this better than iron wire, as we can here produce transversal instead of longitudinal, but neither shows any trace of the currents produced by molecular magnetism. I have made many experiments with wires and bars thus magnetised, but as the effect in every case was negative when freed from experimental errors, I will not mention them; but there is one very interesting proof which the instrument gives, that longitudinal magnetism first passes through its molecular condition before and during the discharge or recomposition of its magnetism. For this purpose, using no battery, I join the rheotome and telephone to the coil, the wire having no exterior circuit. If I strongly magnetise the two ends of the wire, I find by rapidly moving the coil that there is a Faradaic induction of 50° at both poles, but very little or none at the centre of the wire; now fixing the coil at the central or neutral point of the wire and listening intently, no sounds are heard, but the instant I give a slight elastic torsion to the free pole, a rush of electric tertiary induction is heard, whose value is 40°. Again, testing this wire by moving the coil, I find only a remaining magnetism of 10, and upon repeating the experiment of elastic torsion I find a tertiary of 5; thus we can go on gradually discharging the wire, but its discharge will be found to be a recomposition, and that it first passed through the stage I have mentioned.

Heat has a very great effect upon molecular magnetic effects. On iron it increases the current, but in steel the current is diminished. For experimenting on iron wire, which gave a tertiary current of 50° positive (with a torsion of 20°), upon the application of the flame of a spirit-lamp the force rapidly increases (care being taken not to approach red-heat) until the force is doubled, or 100 positive. The same effects were obtained in either direction, and were not due to a molar twist or thermo-current, as if care had been taken to put on not more than 10° of torsion, the wire came back to zero at once on removal of the torson. Hard tempered steel, whose value was 10° whilst cold, with a torsion of 45°, become only 1° when heated, but returned (if not too much heated) to 8° when cold. I very much doubted this experiment at first, but on repeating the experiment with steel several times I found that on heating it I had softened the extreme hard (yellow) temper to that of the well-known blue temper. Now at blue temper, hot, the value of steel was but 1° to 2°, whilst soft iron of a similar size gave 50° of force cold, and 100° at red heat. Now as I have already shown that the effects I have described depend on molecular elasticity, it proves at least, as far as iron and steel are concerned, that a comparatively perfect elastic body, such as tempered steel, has but slight molecular elasticity, and that heat reduces it, but that soft iron, having but little molar elasticity, has a molecular elasticity of a very high degree, which is increased by heat.

The objects of the present paper being to bring the experimental facts before the notice of the Royal Society, and not to give a theoretical solution of the phenomena, I will simply add that if we assume with Poisson that the paths of the molecules of iron are circles, and that they become ellipses by compression or strain, and also that they are capable of being polarised, it would sufficiently explain the new effects.

Joule has shown that an iron bar is longer and narrower during magnetisation than before, and in the case of the transverse strain the exterior portions of the wire are under a far greater strain than those near the centre, and as the polarised ellipses are at an angle with the molecules of the central portions of the wire, its polarisation reacts upon them, producing the comparatively strong electric currents I have described.

SCIENTIFIC SERIALS

Transactions and Proceedings of the Botanical Society of Edinburgh, vol. xiv., part 1, 1881, contains—Address by the president, Dr. T. A. G. Balfour (this address gave brief obituary of J. M'Nab, Sir W. C. Trevelyan, Dr. M. Bain, Prof. Grisebach, A. Forbes, A. J. Adie, Dr. J. Cumming, Karl Koch, Dr. J. Murchison, Dr. D. Moore, P. S. Robertson, Wm. Mudd, Dr. J. F. Th. Irmisch, S. Hay, Dr. M. A. E. Wilkinson, Rev. W. B. Cunningham, E. V. Sandilands, and A. Graham).—Dr. W. Traill, on the growth of *Phormium tenax* in the Orkney Islands.—Wm. Gorrie, on the hardiness of New Zealand plants (1878-79).—Prof. G. Lawson, on British-American species of Viola.—S. Grieve, flora of Colonsay and Oransay.—Jas. Blaikie, botanical tour in Engadine.—Sir R. Christison, on the measurement of trees.—Prof. J. H. Balfour, on *Rheum nobile*.—P. M. Thomson, the flowering plants of New Zealand, and their relation to the insect fauna.—J. Sadler, on the flowering of *Yucca gloriosa*.—Prof. Dickson, on the septa across the ducts in *Bougainvillea glabra* and *Testudinaria elephantipes*.

Proceedings of the Linnean Society of New South Wales, vol. v., parts 1 and 2 (1880).—F. M. Bailey, medicinal plants of Queensland; on Queensland ferns, with descriptions of two new species; on a new species of Nepenthes.—M. A. Haswell, on some Queensland Polyzoa, plates 1 to 3; on some new Amphipods, plates 5 to 7.—Wm. Macleay, on a new species of Galaxias, with remarks on the distribution of the species; on a new species of Otolithus and of Synaptura.—Rev. E. T. Woods and F. M. Bailey, on the fungi of New South Wales and Queensland.—Rev. E. T. Woods, on the littoral marine fauna of North-East Australia; on a fossiliferous bed at the mouth of the Endeavour River; on the habits of some Australian Echini.—E. P. Ramsay, on a new species of Oligorus; note on *Galeo-*

cerdo Rayneri.—Prof. Ralph Tate, rectification of the nomenclature of *Purpura anomala*, Angas.—E. Meyrick, descriptions of Australian Microlepidoptera; parts 3 and 4, Tineina.—J. Brazier, on a new variety of *Bulimus Caledonicus*.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, March 17.—Prof. Roscoe, president, in the chair.—The following papers were read:—On the volume of mixed liquids, by F. D. Brown. The author has determined with very great care the alteration in volume which takes place when various liquids are mixed. The liquids experimented with were carbon disulphide and benzene, carbon disulphide and carbon tetrachloride, carbon tetrachloride and benzene, dichlorethane and benzene, dibromethane and benzene, and carbon tetrachloride and toluene. The experiments were made at 20° C. The author concludes that these changes of volume are dependent on the chemical character of the molecules, and not on such physical properties as vapour tension, molecular volume, &c.—On the action of alcohol on mercuric nitrate, by R. Cowper. When mercury is dissolved in twelve times its weight of nitric acid (1·3), the solution allowed to stand until all nitrous fumes have escaped, and twelve parts by weight of pure alcohol added, a crystalline precipitate is formed, with or without heating, which the author has investigated; it has the constitution $(C_2H_2Hg_3O_2)(NO_3)_2$; he has also prepared the hydrate and oxalate of the dyad radical $(C_2H_2Hg_3O_2)$.—On boron hydride, by F. Jones and R. L. Taylor. Magnesium boride is first prepared by heating a mixture of recently-ignited boric anhydride, with twice its weight of magnesium dust, in a covered crucible. On treating the magnesium boride with hydrochloric acid, boron hydride is obtained, always however mixed with a large excess of hydrogen. Its composition is probably BH_3; it resembles in many of its properties arsine (AsH_3) and stibine (SbH_3).—On the action of aldehydes on phenanthraquinone in presence of ammonia, by F. R. Japp and E. Wilcock.—On the action of benzoic acid on napththaquinone, by F. R. Japp and N. H. J. Miller.—Note on the appearance of nitrous acid during the evaporation of water, by R. Warington. The author proves that the nitrous acid is always derived from the atmosphere or from the products of combustion from the source of heat used for evaporating; he also gave some account of the marvellously delicate test proposed by Griess for nitrous acid. The solution is acidified, and some sulphanilic acid with some hydrochlorate of naphthylamin added; if nitrous acid be present, equal to one part of nitrogen in 1000 millions of water; a rose-red tint is developed.—On the sweet principle of *Smilax glycophylla*, by Dr. Wright and Mr. Rennie.—Note on usnic acid and some products of its decomposition, by the late J. Stenhouse and C. E. Groves.—On the absorption of solar rays by atmospheric ozone, and on the blue tint of the atmosphere, by W. N. Hartley. The author concludes that the higher regions of the atmosphere contain much more ozone than the layers near the earth's surface, and that the blue tint of the atmosphere is largely due to ozone.—On the nature of certain volatile products contained in crude coal-tar benzenes, by Watson Smith.—On New Zealand Kauri gum, by E. H. Rennie. On distillation this gum yields a terpene, boiling at 157°–158°.

Geological Society, March 9.—Robert Etheridge, F.R.S., president, in the chair.—Robert Thompson Burnett, William Erasmus Darwin, Charles James Fox, and the Rev. T. Granger Hutt were elected Fellows of the Society.—The following communications were read:—Description of parts of the skeleton of an Anomodont reptile (*Platypodosaurus robustus*, Ow.); Part II. The Pelvis, by Prof. Owen, C.B., F.R.S. In this paper the author described the remains of the pelvis of *Platypodosarus robustus*, which have now been relieved from the matrix, including the sacrum, the right "os innominatum," and a great part of the left ilium. There are five sacral vertebræ, which the author believes to be the total number in *Platypodosaurus*. The neural canal of the last lumbar vertebra is 8 lines in diameter, and of the first sacral 9 lines, diminishing to 6 lines in the fifth, and indicating an expansion of the myelon in the sacral region, which is in accordance with the great development of the hind limbs. The sacral vertebræ increase in width to the third; the fourth has the widest centrum. This coalescence of the vertebræ justifies the consideration of the mass, as in Mammalia, as one bone or "sacrum," which may be regarded as approaching in shape that of the Megatherioid mammals, although including fewer vertebræ. Its length is 7½ inches; its greatest breadth at the third vertebra, 5½ inches. The ilium forms the anterior and dorsal walls of the acetabulum, the posterior and postero-ventral walls of which are formed by the ischium and pubis. The diameter of its outlet is 3 inches, the depth of the cavity 1½ inch; at its bottom is a fossa 1½ inch broad. The foramen is subcircular, 1 inch in diameter. The ventral wall of the pelvic outlet is chiefly formed by the pubis; it is a plate of bone 6 inches broad, concave externally, convex towards the pelvic cavity. The subacetabular border is 7–8 lines thick; it shows no indication of a pectineal process, or of a prominence for the support of a marsupial bone. The author remarks that of all examples of pelvic structure in extinct Reptilia this departs furthest from any modification known in existing types, and makes the nearest approach to the Mammalian pelvis. This is shown especially by the number of sacral vertebræ and their breadth, by the breadth of the iliac bones, and by the extent of confluence of the expanded ischia and pubes.—On the order Theriodontia, with a description of a new genus and species (*Ælurosaurus felinus*, Ow.), by Prof. Owen, C.B., F.R.S. The new form of Theriodont reptile described by the author in this paper under the name of *Ælurosaurus felinus* is represented by a skull with the lower jaw, obtained by Mr. Thomas Bain from the Trias of Gough, in the Karoo district of South Africa. The post-orbital part is broken away. The animal is mononarial; the alveolar border of the upper jaw is slightly sinuous, concave above the incisors, convex above the canines and molars, and then straight to beneath the orbits. The alveolar border of the mandible is concealed by the overlapping teeth of the upper jaw; its symphysis is deep, slanting backward, and destitute of any trace of suture; the length of the mandible is 3½ inches, which was probably the length of the skull. The incisors are $\frac{5-5}{5-5}$, and the molars probably $\frac{5-5}{5-5}$ or $\frac{6-6}{6-6}$, all more or less laniariform. The length of the exserted crown of the upper canine is 12 millims.; the root of the left upper canine was found to be twice this length, extending upwards and backwards, slightly expanded, and then a little narrowed to the open end of the pulp-cavity. There is no trace of a successional canine; but the condition of the pulp-cavity and petrified pulp would seem to indicate renewal of the working part of the canine by continuous growth. The author infers that the animal was monophyodont. *Ælurosaurus* was said to be most nearly allied to *Lycosaurus*, but its incisor formula is Dasyurine. With regard to the characters of the Theriodontia the author remarked that we may now add to those given in his "Catalogue of South African Fossil Reptiles" that the humerus is perforated by an entepicondylar foramen and the dentition monophyodont.—Additional observations on the superficial geology of British Columbia and its adjacent regions, by G. M. Dawson, D.Sc. This paper is in continuation of two already published in the Society's *Journal* (vol. xxxi. p. 603, and vol. xxxv. p. 89). In subsequent examinations of the southern part of the interior of British Columbia the author has been able to find traces of glaciation in a north to south direction as far as or even beyond the 49th parallel. Iron Mountain, for instance, 3500 feet above the neighbouring valleys, 5280 feet above the sea, has its summit strongly ice-worn in direction N. 29° W. to S. 29° E. Other remarkable instances are given which can hardly be explained by local glaciers; boulder-clay is spread over the entire district; terraces are cut in the rearranged material of this, bordering the river-valleys, and at greater elevations expanding over the higher parts of the plateau and mountains. At Mount It-ga-chuz they are 5270 feet above the sea. The author considers that the higher terraces can only be explained by a general flooding of the district. Some of the wide trough-like valleys of the plateau contain a silty material which the author regards as a glacial mud. North of the 54th parallel and west of the Rocky Mountains similar evidence of glaciation is obtained; erratics are found in the Peace and Athabasca basins. The fjörds of British Columbia are extremely glaciated, the marls being generally in conformity with the local features; terraces are scarce and at low levels. The Strait of Georgia was filled by a glacier which overrode the south-east part of Vancouver's Island; evidence is given to show that this ice came from the neighbouring mountainous country. Queen Charlotte's Island shows evidence of local glaciation. Boulder-clays and stratified drifts are found, with occasional Arctic shells. The author considers that the most

probable explanation of the phenomena of the whole region is to suppose the former existence of a great glacier mass resembling the inland ice of Greenland, and that the Glacial period was closed by a general submergence, during which the drifts were deposited and, at its close, the terraces cut.

Photographic Society, March 8.—J. Glaisher, F.R.S., president, in the chair.—Papers were read by Mr. Payne Jennings on art photography. It was asserted that unfavourable criticisms, both from artists and the press, had been the result from the exhibition of works which deserved such severity, and that to raise the status of art in photography more attention must be given to art-rules.—Also by Mr. Edwin Cocking, on notes on photography and art. An incisive comparison was drawn between the art of the painter and that of the photographer, showing the essential difference between the two in the production of a pictorial work, both in the *modus operandi* of production and the individuality capable of being infused into each result. Also that art in photography required a totally different training to that necessary for the painter, and that the time had arrived when special instruction by a thoroughly organised school for art photography had become absolutely necessary.

Institution of Civil Engineers, March 22.—Mr. Abernethy, F.R.S.E., president, in the chair.—The paper read was on the comparative endurance of iron and mild steel when exposed to corrosive influences, by Mr. D. Phillips, M. Inst. C.E.

PARIS

Academy of Sciences, March 21.—M. Wurtz in the chair.—The following papers were read:—On determination of the masses of mercury, Venus, the Earth, and of solar parallax, by M. Tisserand.—Observations of Faye's comet, at Paris Observatory, by MM. Tisserand and Bigourdan.—On the possibility of making sheep refractory to anthrax through preventive inoculations, by M. Pasteur, with MM. Chamberland and Roux. M. Pasteur controverts M. Toussaint's views on the subject, and says his method is very uncertain.—The vaccine matter of anthrax, by the same. A wholly harmless bacterium can be got from the most virulent by cultivation in animals different from those apt to take the disease. There are as many distinct germs as there are different kinds of virulence.—Researches on formic ethers, by MM. Berthelot and Ogier. They are formed with absorption of heat.—New navigation-maps, giving both the direction and force of the wind in the Indian Ocean, by M. Brault. Meteorologically the parts of that ocean above and below the equator are distinct (and the author indicates how).—On the operations of the Syndical Association of the Beziers Arrondissement to oppose phylloxera, by M. Janssan.—Report on the work of the Council of Public Hygiène and Salubrity, by M. Brezançon.—On the surface with sixteen singular points and Θ functions with two variables, by M. Darboux.—On the functional determinant of any number of binary forms, by M. Le Paige.—On the decomposition into primary factors of uniform functions having a line of essential singular points, by M. Picard.—On certain simultaneous linear differential equations with partial derivatives, by MM. Picard and Appell.—On generator polygons of a relation between several imaginary variables, by M. Lecornu.—Solution of a general problem on series, by M. André.—On linear differential equations with algebraic integrals, by M. Poincaré.—On the distribution of energy in the normal solar spectrum, by Prof. Langley. The total heat coming from the sun to the earth is much greater than has been believed (even in estimates accused of exaggeration). If the totality of the solar radiations reached us we should have a sensation of blue rather than white. (The author studied the absorption for each ray).—On a synthetic apparatus reproducing the phenomenon of circular double refraction, by M. Gouy. This consists of a number of thin and narrow rectangular lamellæ of crystal placed side by side like floor-boards, and cemented between two glass plates. In a given direction the optic axis of each band forms a constant angle with the preceding one. A half-wave plate is placed above.—On radiophony with selenium, by M. Mercadier. The sounds here result chiefly from the luminous rays from the limit of blue to extreme red, and even a little in infra-red, the maximum being in the yellow.—Experiments at the Creusot works in optical measurement of high temperatures, by M. Crova. The spectropyrometer is proved practically useful.—On the electromotive force of the voltaic arc, by M. Le Roux. With a galvanometer of great resistance and a single contact operated with the hand, one may prove the difference of potential of the carbons even $\frac{2}{10}$ of a second after cessation of the current. The phenomenon is probably thermo-electric.—The hissing of the voltaic arc, by M. Maudet. The difference of potential between the carbons is very great when the arc is silent, very small when it hisses.—On magic mirrors of silvered glass, by M. Laurent. The magic effect can be had through the mode of mounting of the mirror.—On the flow of gases, by M. Neyreneuf. The laws of this may be verified by a method like that for determining electric resistances.—On new combinations of hydrobromic and hydriodic acid with ammonia, by M. Troost.—Action of hydrochloric acid on chloride of lead, by M. Ditte.—Action of sulphuric acid newly heated to 320°, and oils, by M. Maumené.—On a new means of analysis of oils, by the same. This consists in treating a measured quantity of oil with one of a titrated aqueous solution of caustic alkali.—Separation of oxide of nickel and oxide of cobalt, by M. Delvaux.—On a process of industrial manufacture of carbonate of potash, by M. Engel.—On some complex compounds of sulphur and nitrogen, by M. Demarçay.—On tar from cork, by M. Bordet. It contains more hydrocarbons than tar from coal, and less of oxygenated substances than tar from hard woods.—On the fermentation of urea, by M. Richet. The stomachal mucus of animals in general causes ammoniacal fermentation of pure urea.—Physiological and therapeutical properties of cedrine and valdivine, by MM. Dujardin-Beaumetz and Restrepo.—Physiological action of *Erythrina corallodendron*, by MM. Bochefontaine and Rey.—On lesions of the bones in locomotor ataxy, by M. Blanchard.—On the presence of trichina in adipose tissue, by M. Chatin.—On the virulent state of the fœtus in sheep dead from symptomatic anthrax, by MM. A. Arloing, Cornevin, and Thomas—Illusion relative to the size and distance of objects from which one withdraws, by M. Charpentier. The objects seem to enlarge on approach.—On the organs of taste of osseous fishes, by M. Jourdan.—Toxical power of pancreatic microzymas in intravenous injections, by MM. Béchamp and Balten.—Human bones found in the diluvium of Nice—the geological question, by M. Desor. The deposit (at Carabacel) belongs to the category of strata contemporary with the erosion of tertiary plateaux.—Description of the bones, by M. Niepce.—Determination of the race, by M. de Quatrefages. It seem to be the same as that of the men of Cro-Magnon.—On a new genus of primary fish, by M. Gaudry. MM. Riche found it in the Permian of Igornay. It is remarkable for the great size of its ribs, and is called *Megapleuron Rochei*. It had lozenge scales.—On the existence and characters of the Cambrian formation in the Puy-de-Dôme and Allier, by M. Jullien.—General law of formation of mineral waters; application to Greoux (Basses-Alpes), by M. Dieulafait.—On the discovery at Noir-Montiers (Vendée) of the Eocene flora with *Sabalites Andegavensis*, Sch., by M. Crié.—Observations on variations of temperature of the human body during movement, by M. Villari. The results agree with M. Bonnal's.

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